Trends in the Unequal Pay of Women and Men Across Three British Generations

Jenny Neuburger
Institute of Education, University of London

March 2010

Abstract

Since 1970, women's employment rates and average pay have increased in real terms and relative to men's. This thesis presents a statistical analysis of trends in women's and men's work and pay across three British generations over the period 1972-2004. This analysis uses longitudinal data from the 1946, 1958 and 1970 British Birth Cohort Studies.

Contributing to the methodological literature, an analysis of the links between different theories of labour market discrimination and alternative measures of unequal pay is presented. This includes a detailed examination of different approaches to treating employment selectivity bias in the analysis of wages. On the basis of this work, two measures of unequal pay are quantified using the cohort data.

The first analysis focuses on trends in women's and men's pay opportunities, taking into account those estimated for the non-working population. The motivation is that low pay opportunities may create work disincentives, particularly for women with children. The results suggest that the cross-cohort increase in women's relative pay opportunities is understated in the pay trends for employees.

The second analysis looks at unequal pay for women and men with similar levels of education and experience. The results suggest that unequal treatment has decreased across the cohorts, but not disappeared. In the 1970 cohort, even women who had not had children by the age of 34 were paid less, on average, than similarly qualified men. Women who spent time out of work or who worked part-time after having children experienced decreases in their pay relative to men and to other women.

The thesis concludes that gender inequality has reduced across three British generations, but that it persists for the youngest. The lasting shift toward more equal pay since the introduction of the 1970 Equal Pay Act is evidence that legislation makes a difference.

The work presented in this thesis is my own, other than work that is directly quoted and attributed.

Word count (excluding bibliography): 76,550

Contents

List of Figures

List of Tables

Acknowledgements

I especially want to thank Heather Joshi for her support over four years. Heather's commitment to researching gender inequalities and to applying this knowledge to policy advice has been an inspiration. I have been fortunate to have had two such engaged and encouraging supervisors as Heather Joshi and Diana Kuh.

I want to thank Jony Hudson for his intellectual and emotional support. With logic, humour and flair, Jony has engaged with me in good discussions on the questions covered in this thesis and has made an important contribution to the way that I think about statistics.

I have benefited from discussions and comments on drafts of the thesis from family, friends and colleagues. I would especially like to thank Louisa Neuburger, Rebecca Allen and Augustin de Coulon for reading a final draft and helping me to improve it. Mark Killingsworth, Anna Vignoles, Lorraine Dearden, Charley Greenwood, Murrel Neuburger, Noel Hillmann, Tom Fisher, Tania Burchardt and Meg Callanan have also made particularly useful comments at different points over the last four years.

Thanks are also due to everyone who works at the Centre for Longitudinal Studies and at the MRC National Survey of Health and Development who are engaged in the enormous task of running the cohort studies. On top of their other administrative work, Wendy Robins, Magdalen Meade and Alexandra Frosch do a valuable job of keeping doctoral students engaged in the life of the department and faculty. Imran Shah, Jon Johnson, Brian Dodgeon and Maggie Hancock have particularly helped me with accessing data.

Chapter 1

Introduction

This thesis is concerned with the questions of how unequal labour market opportunities affect women's and men's relative pay over their working lives and with how inequalities have changed across three British generations since the 1970s. For most people, what they earn, relative to others, is a key marker of status in the workplace and reflects the level of skill, responsibility and power involved in the job they do. More than three decades since equal pay legislation was introduced in Britain, women are still likely to earn less than men. The legal principle of equal pay for equal work applies to women and men working for the same employers in the same types of jobs, but not to those either working in different sectors or working in the same field at different levels of seniority. Whilst women's and men's positive and different decisions about childcare, occupation and hours of work may be part of the story, much evidence suggests that unequal treatment persists, albeit in reduced and less direct forms than in the 1970s.

In the current UK labour market, there is evidence that a gender gap in pay emerges over the first ten years in work after leaving full-time education (Manning and Swaffield, 2008). An unexplained gap in pay emerges even for women who work full-time continuously and do not have children. This unexplained gap may arise in part from different job choices and unobserved differences in ability and job commitment, but may also stem from indirect discrimination, placing limits on women's relative opportunities to enter certain occupations, undertake professional training, take on responsibility at work and get promotions. Decisions within couples about which parent should leave their job or reduce their hours of work to look after children are not simply a matter of choice, but are also financially constrained by which parent earns more. There is evidence that motherhood has immediate and long-term negative effects on women's hours of work and pay (Brewer and Paull, 2006). Time spent in part-time work also has negative impacts on long-term job prospects and earnings (Connolly and Gregory, 2009). The negative impacts of having children and part-time working on pay are not necessarily proportionate to the associated loss of productivity and relative experience. The result may be distribution of work and pay amongst women and men later on in life that is unequal and inefficient, with women ending up more likely to have jobs that are worse paid and which under-use their skills.

Alongside evidence of persistent gender inequalities in the current UK labour market, there is also strong evidence that gender inequality has decreased since the 1970s. Women's hourly wages have increased, in real terms and relative to men's wages. The implementation of equal pay and sex discrimination legislation in 1975 led to increases in women's relative average pay following decades of no change (Neuburger, 1984; Zabalza and Tzannatos, 1985; Joshi et al., 1985; Manning, 1996). The introduction of the statutory maternity leave in 1975 and extensions to maternity rights over the 1980s led to an increase in rates of employment amongst mothers of young children (Gregg et al., 2007).

The first of the substantive research questions addressed in this thesis is motivated by this simultaneous increase in women's relative pay and rates of employment. The observed increase in relative pay may differ from trends in pay opportunities for the whole population, since the proportion of women in work has simultaneously increased and, with it, the relative characteristics of the female workforce have changed. Using data from the Family Expenditure Survey (FES) for 1978 and 1998, Blundell et al. (2007) analysed the effects of changes in the composition of the female and male workforce on gender differentials. They found evidence that changes in the composition of the workforce, induced by changes in employment participation, may conceal part of the improvement in the labour market position of women since 1978. This thesis considers the same question, focusing on trends across the three British birth cohorts over three decades from 1972, before equal pay legislation came into force, to 2004.

The issue explored in the second part of this thesis is that the decrease in the gender pay gap since the 1970s is not, on its own, evidence that the unequal treatment of similarly skilled women and men has reduced. Part of the decrease in the pay gap stems from women's increased levels of education and labour market experience. The exercise here is to quantify the effects of unequal labour market treatment on the pay of women and men with similar levels of education and experience. It takes as its starting point the substantial body of evidence for the cohorts (Joshi and Newell, 1989; Joshi and Paci, 1998; Makepeace et al., 1999; Joshi et al., 2007). In these studies, the effects of unequal treatment on pay are quantified using estimates of gender differences in pay not accounted for by gender differences in qualifications and labour market experience. The key finding is of a cross-generational decrease in unequal treatment. Taking a similar approach, the analysis in this thesis uses new survey data for the 1970 cohort at age 34 (in 2004) to extend the picture for this cohort and also presents estimates based on previously unused survey data from the 1946 cohort at age 43 (in 1989) to explore trends for older workers.

In the same way that cross-generational comparisons of women's labour market position are complicated by simultaneous changes in employment participation and pay, the picture of life-cycle pay opportunities is partially obscured by composition effects, caused by the exit and return to work of different groups of women at different ages. To assess changes in relative pay for the same groups of women and men with age, the third piece of work in this thesis uses the longitudinal aspect of the cohort datasets. The starting point for this investigation is the work on gender differences in wage growth over the life-cycle draws on the work of Manning and Petrongolo (2008), Brewer and Paull (2006) and Connolly and Gregory (2009). Using longitudinal evidence from the British Household Panel Study (BHPS), Manning and Swaffield (2008) found that a substantial pay gap emerges over the first ten years of women's and men's working lives, including amongst women who have not had children and have worked full-time continuously. Using data from the BHPS (1991-2003) and from the Family and Children Study (1999-2003), Brewer and Paull (2006) found that having a baby marked a sharp drop in women's participation in paid work and the start of a ten-year decline in their wages, relative to men's. Using panel data from the New Earnings Survey Panel Dataset (NESPD) for 1975-2001, Connolly and Gregory (2009) found that women who switched to part-time jobs, combined with the effects of switching employer and occupation, had permanently lower earnings trajectories. The analysis presented in this thesis focuses on life-cycle wage growth between the ages of 23 and 42 for the 1958 cohort and between the ages of 26 and 34 for the 1970 cohort. This analysis is the first to use the longitudinal earnings data from the cohorts to look at gender inequalities. Although wage data usable for this purpose have only been collected at three ages from these two cohorts, and compare unfavourably to the BHPS in this respect, the data go back to 1981 (ten years before the BHPS started) and contain substantial, longitudinal samples for the the two separate cohorts.

1.1 Overview of thesis

The following sections of this chapter set out the policy and historical context, covering the key empirical evidence.

Chapter 2 considers different theoretical approaches to the question of how to measure unequal pay. A comprehensive neo-classical approach to labour and household economics defines unequal pay as a difference in pay between equally qualified and experienced women and men doing the same types of job. This view implies that decisions about occupation, family care and hours of work are positive, rather than constrained, choices. A number of less straightforward forms of unequal treatment are predicted by other labour market theories. For example, if employers tend to promote men over women to the higher-grade positions, this aspect of unequal treatment will be missed in comparisons of women and men in the same jobs. Another aspect that would be missed would be the lower pay of women in female-dominated sectors, where employers might pay a low rate and offer little training, relying on the fact that many of their female employees are limited in their job search by childcare arrangements. Taking the long view, it may even be that women's and men's expectations and aspirations about work and family life are shaped by unequal employment opportunities and that they adjust their behaviour accordingly. Theoretical approaches to these issues vary in how much emphasis they place on the role of individual choice and how far they view individual choices as being constrained by an unequal distribution of power in the labour market.

The first part of the analysis presented in this thesis draws on the last and broadest of these concepts, starting from the idea that decisions about employment, childcare and education are interdependent and are shaped by unequal opportunities in the labour market. The substantive question addressed is: how have women's and men's pay opportunities changed over time, when the standard analysis of earnings is extended to take into account the potential earnings of people not in work? The motivation for this question is that, by looking only at the average pay of employees, we may get a partial and distorted picture of opportunities. The distortion comes from the fact that wages are only observed for those who decide to enter employment, whilst employment decisions are in turn likely be affected by the potential wage i.e. the wage that an individual could earn if they took a paid job. This interdependence is what makes the distribution of pay opportunities, and not just pay, important and interesting.

Chapter 3 surveys the methodological literature, focusing on the different theoretical models and assumptions that have been used to estimate unobserved potential wages for non-working individuals. Chapter 4 introduces the British Birth Cohort Studies, including a detailed account of the work done to construct variables on hourly earnings, employment experience and educational qualifications. It also provides information on the representativeness of the studies, on data quality and on the statistical treatment of these issues. Chapter 5 presents the results of an analysis using the cohort data.

The empirical analysis presented in chapter 5 uses an imputation method, similar to methods used previously by Blau and Kahn (2006a) and Olivetti and Petrongolo (2008). Missing potential hourly wages for non-working individuals are replaced with the observed wages of otherwise-similar working individuals. Similar individuals are identified as those born in the same year, of the same sex and of the same age, also with a similar propensity-score (probability of being in work) based on their childhood ability scores, social class and family backgrounds and child-bearing and employment histories. The large and detailed files of information on members of the British Birth Cohort Studies make these studies an ideal source of data for this statistical exercise.

Chapters 6 and 7 start from a narrower, more standard concept of unequal pay as a difference in pay for equal productive characteristics. The methodological and empirical difficulty here lies in selecting characteristics to measure individual productivity, which we are not able to directly observe or quantify. It is standard in studies of this kind to use proxy measures of educational attainment and employment experience. However, measurement problems arise from the correlation between employment experience and other, confounding and discriminatory, influences on pay. The empirical issues involved in quantifying trends are also challenging, given that employment rates and distributions of educational qualifications and pay in the population have changed, and have changed differently for women and men.

In chapter 7, standard decomposition methods are used to quantify unequal treatment across the three cohorts, adding new estimates to previous work done on these cohorts (Joshi and Newell, 1987; Joshi and Paci, 1998; Makepeace et al., 1999; Joshi et al., 2007). In addition, life-cycle pay trends are estimated for matched sub-samples of women and men. The pay trajectories of sub-samples of women who have worked full-time fairly continuously and of those who have not had children are also examined, relative to those of men with similar qualifications and family backgrounds.

Chapter 8 draws together the empirical findings from the analyses carried out and discusses the theoretical and methodological implications of these results. Likely trends for current and future generations of women and men are considered, alongside the possibilities for policy to shape these trends.

1.2 Equal pay and sex discrimination legislation in Britain

The Equal Pay Act was introduced in Britain in 1970 and came into force on the 29^{th} December 1975. Its aim was to end sex discrimination in regard to pay and terms of employment. It established an individual right to equal treatment in contracted terms of employment for the same or equivalent work, covering jobs which were different to those of the opposite sex, but had been judged to be similar, in terms of skill, effort and responsibility, under a job evaluation scheme. The comparison of pay and terms of employment was limited to women and men working for the same employer. The Act gave individuals the right to make an application to an industrial tribunal if they believed that they were not being treated equally. Provisions were made for the referral of collective agreements and orders made by wages councils to the Central Arbitration Committee.[1] An amendment to the Equal Pay Act was introduced in 1983 to extend the principle of equal pay to jobs of 'equal value'. However, this amendment still required a female employee making an equal pay claim to compare herself with a male employee in the same establishment (or vice versa).[2]

The Sex Discrimination Act also came into force in 1975. The Act made it unlawful for an employer to treat a woman less favourably than a man, on the ground of sex, including in recruitment and in non-contractual benefits of employment, with the exclusion of benefits relating to retirement or death. The Act also covered discrimination against married versus single women and indirect discrimination, such as the inclusion of employment requirements or conditions with which fewer women than men could comply. The Equal Opportunities Commission was set up and charged with the duties of working towards the elimination of discrimination and of promoting equality of opportunity between men and women more generally.

The Employment Protection Act of 1975 introduced the first statutory paid maternity leave. This covered a Right of Reinstatement after a period of unpaid leave. The right only extended to women who had worked full-time for the same employer for at least two years or part-time for five years. Since its introduction, the qualifying conditions for maternity leave have been reduced and the generosity of maternity pay has been increased. The Right to Request Flexible Working was introduced in 2003. In addition, parents of children aged under six (or of children with disabilities under eigh-

[1] Collective agreements and wage orders covered nearly two-thirds of the working population in the early 1970s (Zabalza and Tzannatos, 1985, chap. 3).

[2] The Equal Pay (Amendment) Regulations were introduced to comply with European Community (EEC) laws. In 1982, the European Court of Justice ruled that Britain had failed to fulfil her obligations under the Treaty of Rome with respect to the principle of 'equal pay for work of equal value'.

teen) were given right to apply to work flexibly and employers have a duty to consider applications seriously.

European law led to a gradual extension of employment rights of part-time employees over the 1990s. In 2000, the Part-Time Workers Regulations were introduced, covering rates of pay and conditions of employment. Also affecting the low pay of many part-time employees, as well as a smaller fraction of full-time employees, was the introduction of a National Minimum Wage in April 1999.

A new Single Equality Act is expected to come into force from 2010. The Bill was published in April 2009. The aim is to simplify the existing equalities legislation. There are also new proposed measures, including introducing compulsory gender pay reports in 2013 for a minority of employers and specific rights for carers and new mothers.

1.3 Trends in women's and men's earnings and employment

Since the introduction of the Equal Pay Act, women's earnings have increased substantially, relative to men's. Figure 1.1 illustrates the trends in women's mean hourly earnings relative to men's for full-time employees. This reveals a sharp increase in women's relative pay around the time of the implementation of the Equal Pay Act. Several studies have found evidence that the Equal Pay Act was directly responsible for the increase in women's average pay over this period, following decades of no change (Neuburger, 1984; Zabalza and Tzannatos, 1985; Joshi et al., 1985; Manning, 1996).[3] The legislation was not designed to raise the pay of women working in part-time jobs in different sectors from men. Between 1975 and 2008, the mean hourly earnings of women in part-time work increased, from 56 per cent to 63 per cent of those of men in full-time work, but at a lower rate than those of women in full-time work (New Earnings Survey 1975, Annual Survey of Hours and Earnings 2008).

The cross-generational increase in women's relative earnings has been concentrated amongst women in the twenties and thirties. Figure 1.2 shows ratios of women's to men's median pay for a cross-section of full-time employees of different ages in 1975 and 2006. These snapshots of the labour market overstate the average pay of older

[3] Greenhalgh and Stewart (1985) found that women and men who in the labour market continuously over the decade before equal pay legislation was introduced benefited from changes in the occupational structure, but that the majority of married women entering the labour market filled low paid part-time jobs.

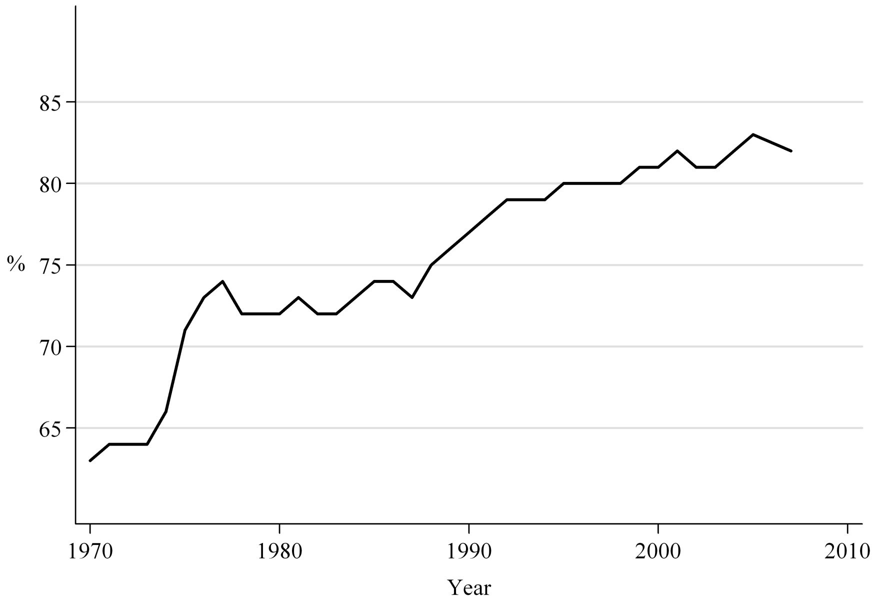

Figure 1.1: Women's mean hourly earnings as a % of men's in full-time work, 1970-2007. Sources: New Earnings Survey 1970-2002, Annual Survey of Hours and Earnings 2003-2007, Office for National Statistics

female employees, since they exclude part-time employees, who comprise 40 per cent of the female workforce and a larger fraction of older female employees.

Women's and men's rates of employment have converged since the 1970s (Figure 1.3). This is a feature of labour markets in most OECD countries (see Boeri et al., 2005). In Britain, men's rates of employment have steadily decreased, not fully recovering after sharp drops during the recessions of the early 1980s and early 1990s. Increasing numbers of working-age men have become long-term economically inactive (Disney and Webb, 1991; Faggio and Nickell, 2003). Rates of economic inactivity for working-age men increased from around 5 per cent in 1971 to 16 per cent in 2008, whilst unemployment rates increased from around 4 per cent to around 6 per cent, with temporary increases to around 12 per cent during the recessions of the early 1980s and 1990s (ILO unemployment rates, Labour Force Survey).[4] The increase in economic inactivity has been concentrated amongst men without qualifications and with a chronic health problem or disability (Faggio and Nickell, 2003) and has been greater in regions

[4] At the time of writing in August 2009, the unemployment rate is 7.8 per cent and projected to rise as Britain enters a recession (Office for National Statistics).

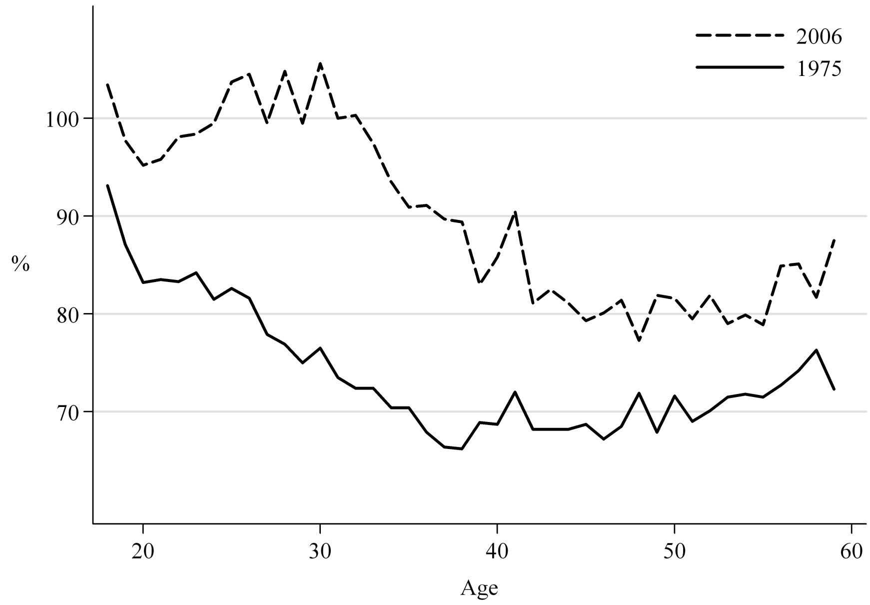

Figure 1.2: Women's median hourly earnings as a % of men's in full-time work at each age, 1975 and 2006. Source: Office for National Statistics, 2008c, derived from Figure 5.10

with high previous rates of unemployment, particularly former industrial heartlands in northern England, South Wales and central Scotland (Fothergill, 2001).

In contrast, women's rates of employment have increased steadily, with a large increase in the mid-1980s and temporary dips during the recessions before and after this. The increase in women's employment has been concentrated amongst women in their twenties and thirties (see, for example Blundell et al., 2007, Figure 2). The increase is composed of an increase, firstly, in the proportion of women who do not have children, have fewer children or who have them later and secondly, in employment rates amongst married and cohabiting mothers.

The average number of children born to each woman over the course of her life was more than two for the generations born before 1958 and dropped below two for generations born afterwards (Office for National Statistics, 2008a, table 10.2). The decline in births in the 1970s coincided with the increase in women's potential earnings (Joshi et al., 1985; Ermisch, 1979). Murphy (1993) argued that the diffusion of the oral contraceptive pill, substituting for less effective forms of contraception, was a contributory factor, drawing attention to the fact that two 'pill scares' coincided with two periods

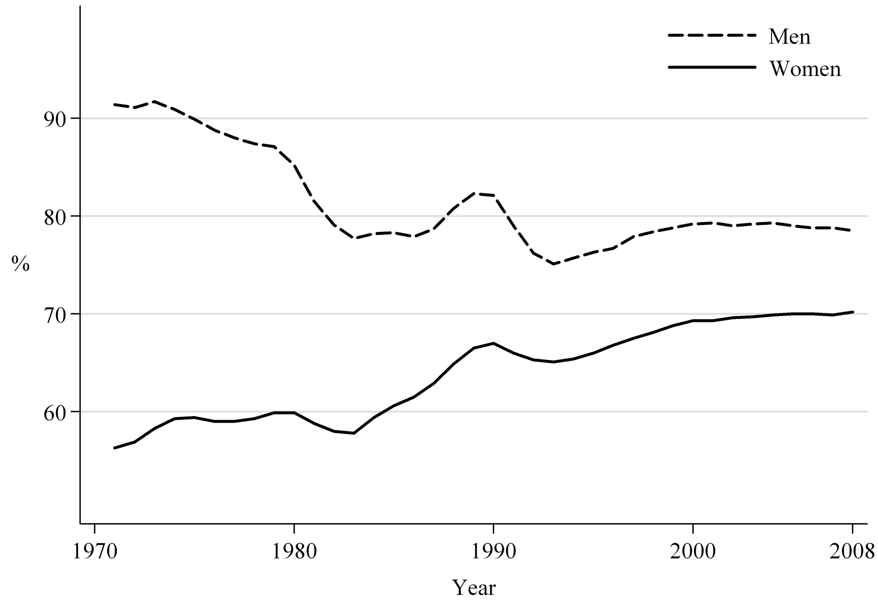

Figure 1.3: Percentage of women and men in employment (women aged 16-59, men aged 16-64). Source: Labour Force Survey, Office for National Statistics

in which the steady decrease in births was checked. The mean age of mothers at a first (live) birth in England and Wales also increased from under twenty-four in 1971 to over twenty-seven by 2006 (Office for National Statistics, 2008b, table 2.17).

The introduction of paid maternity leave in the Employment Protection Act 1975 has been found to be a contributory factor to the increase in mothers' employment rates. Gregg et al. (2007) found that the introduction of maternity leave, and subsequent policy changes to increase the generosity and reduce the qualifications for this leave, contributed to the employment increase among mothers of young children between 1974 and 2000. The authors used repeated cross-sections of the General Household Survey (GHS) to compare the employment patterns of married or cohabiting mothers with those of married or cohabiting women without children across eight quasi-cohorts, finding that relative increases amongst the former group (mothers) coincided with changes in maternity leave legislation.

Another factor contributing to the increase in women's relative earnings and employment has been the increase in their levels of education. Educational opportunities increased for women and men born in the 1950s, with the introduction of comprehensive education under the 1964-1970 Labour Government and the expansion of university places in the 1960s and 1970s.[5] Across two of the British birth cohorts, born in 1946 and 1970 respectively, the proportion with no or very low qualifications when in their early thirties fell from around half in 1978 to a fifth in 2000 for both women and men. Even more striking, the proportion with degree-level qualifications or above increased from just 10 per cent to 32 per cent for women, and from 21 per cent to 31 per cent for men (Makepeace et al., 2003, table 2.1a). Rates of employment have been higher amongst women with qualifications than amongst women without qualifications throughout the post-war period in Britain. The aggregate increase in women's employment rates is associated with the increase in the proportion of women with qualifications (Gomulka and Stern, 1990; Gutiérrez-Domènech and Bell, 2004).

The 1980s and 1990s were also decades of increasing differences in the employment rates of mothers with and without qualifications (Macran et al., 1996; Joshi, 2002). Women and men with higher qualifications have also been increasingly likely to not have children or have children later in life, compared to less-qualified women and men (Joshi, 2002; Kneale and Joshi, 2008; Jenkins et al., 2008).

Differences in the hourly earnings of those with and without qualifications have increased since the early 1980s, as has the dispersion of hourly earnings within educational groups (Machin, 2003; Hills, 2004; Blundell et al., 2007). Figure 1.4 shows the steady increase in the ratio of the 90th to the 10th percentile hourly wage for men and women over the period 1975-2000.[6] The widening dispersion of earnings has also been a feature of the United States labour market and there is a large literature documenting and analysing this phenomenon in the UK (see Machin, 2003) and the US (for a review of the literature, see Katz and Autor, 1999).

Related to the changes in the earnings structure in the UK has been a change in the structure of jobs, with a decline in manufacturing and an increase in service sector jobs. One aspect of this change has been the growth in low-paid and high-paid service sector jobs, and a decrease in middle-level jobs, which is also a feature of the US labour market (see Autor and Dorn, 2009). Goos and Manning (2007) analysed changes in the distribution of jobs in Britain using data from the LFS and NES over the period 1975-1999. Defining a job by occupation and industry codes and ranking jobs on the basis of their median wage at the beginning of the period, they found rises in the proportion of jobs at the top and bottom of the distribution and a decline in the middle. They

[5] The minimum school leaving age was raised to fifteen under the 1944 Education Act and to sixteen in 1973, with preparations for this second change beginning in 1964.

[6] The check in this increase between 1990 and 1995 was associated with lower growth in the top (90th percentile) wages during the recession of the early 1990s.

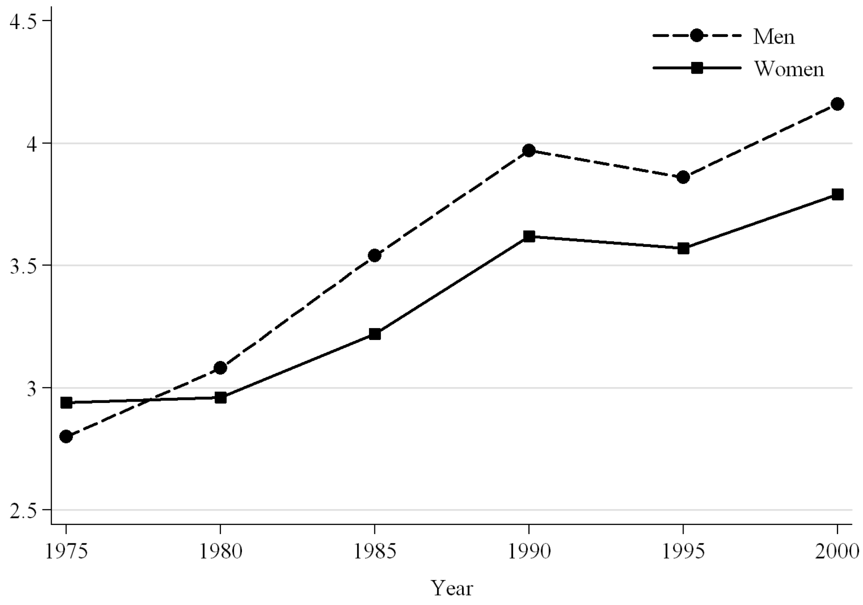

Figure 1.4: Ratio of 90th to 10th percentile of hourly wage distribution for women and men, 1975-2000. Source: Machin, 2003, from figures based on FES data in Table 12.2

also found that the shift in the structure of jobs accounted for a substantial fraction of the rise in wage inequality, alongside increases in within-job and between-job wage inequality.

Manning and Petrongolo (2008) also found that the widening gap between the average pay of women in full-time and part-time jobs since the mid-1970s was associated both with the increasing occupational segregation of full-time and part-time jobs, combined with the impact of increasing wage inequality on relative pay in low-paid part-time occupations. Whilst women in full-time jobs have become more spread through the occupational structure, part-time jobs remain concentrated within low-paying sectors; in 2006, a third of female part-time employees were working in just one of only five occupational groups: sales assistants, cleaners, care assistants, general clerks and educational assistants (Gregory and Connolly, 2008).

The introduction of the National Minimum Wage in April 1999 increased wages at the bottom of the wage distribution and has disproportionately benefited women who are more likely than men to be low paid. Machin et al. (2002) found that the minimum wage had a widespread impact in the residential care home sector, in which a third of workers were paid less than the incoming minimum rate. There is evidence of some narrowing of the gender pay gap at the bottom of the wage distribution (Robinson, 2003b). The impact of the minimum wage further up the wage distribution has been negligible and the impact on the average gender pay gap has been modest (Dickens and Manning, 2004; Robinson, 2003b).

The interdependence of earnings opportunities and employment participation further complicates the picture of change. For men, the drop in relative wages for unskilled work, alongside the weak incentives to come off incapacity benefit once on it, have been suggested as causes of the long-term persistence in economic inactivity over periods of economic growth and job creation (Faggio and Nickell, 2003; Disney and Webb, 1991). The widening of the wage distribution has also altered the relative wage incentives and costs associated with paid working for different groups of mothers. Gregg et al. (2007) found that the increase in mothers' employment rates since the 1970s was greatest amongst highly-qualified mothers. Dividing the mothers into three groups based on their predicted earnings, they found that mothers with the highest potential earnings responded most to the change in maternity legislation in the early 1980s, whilst those with middle earnings increased their rates of employment later, in the late 1980s and 1990s. Mothers in the lowest predicted earning third did not change their employment behaviour. The authors speculated that this could be owing to an increase in childcare costs, in line with the increase in real wages in the bottom third of the distribution.

The role that unequal treatment of women and men in work has played in these changes is a matter upon which opinion is divided. One interpretation is that the decrease in sex discrimination in pay has itself provided more recent generations of women with stronger financial incentives to enter and remain in the workforce, with cumulative positive impacts on their pay. Using the example of the 1970 Equal Pay Act, Neuburger (1984) specifically disputed the 'priced-out' argument i.e. that requiring employers to pay women more would mean that they employed fewer women. He drew attention to evidence that women's employment did not reduce in private-sector industries in which women's pay increased sharply relative to men's. Manning (1996) also presented the employment effects of the Equal Pay Act as a critical piece of evidence in this debate, arguing that the evidence supported the idea that women were being paid below their economic value before the Act and that this form of discrimination was reduced. The empirical evidence on the employment effects of the UK National Minimum Wage in 1999 is mixed. The numbers of low-paid workers employed overall does not appear to have been affected (Stewart and Swaffield, 2004) and Connolly and Gregory (2002) did not find evidence of an impact on women's working hours. In contrast, Stewart and Swaffield (2008) found a small negative impact on working hours and Machin et al. (2002) also found evidence of a small negative impact in the care home sector.

An alternative argument has been made that the widening dispersion of wages and the increase in the availability of low-paid employment was in fact what enabled low-skilled women to enter the workforce over this period in the UK and Europe. This interpretation of the facts implies that women's low productivity is primarily responsible for their low pay and also that there is a necessary historical sequencing of changes, with a decline in gender wage differentials coming only after a closing of the gender employment gap (Boeri et al., 2005, p.2). The work presented in this thesis on the measurement of unequal pay and on empirical trends in Britain is intended to contribute to this debate.

Chapter 2

Different theoretical perspectives on unequal pay

This chapter addresses the central question of how we should define unequal pay of women and men, and considers the theoretical bases for alternative definitions. The legal definition of equal pay for equal work in the UK suggests that the basis for comparison should be women and men in the same jobs. However, if unequal treatment in the labour market also takes the form of barriers to women and men entering the same fields of work or to gaining promotions, we may legitimately be concerned with a broader concept of unequal pay. Alternative views on what constitutes 'unequal', rather than 'justified', differences in women's and men's pay reflect alternative views about which family and employment decisions are positive, active choices and which decisions are constrained or wholly determined by unequal treatment.

The theories discussed in this chapter are grouped under three headings, based on the three alternative interpretations they provide of gender differences in pay. The first part of the chapter surveys theories that emphasise the role of individual choice in nearly all aspects of family and labour market behaviour. The idea that women choose their jobs and design their working lives around raising children is the main explanation for the gender pay gap offered by human capital theory (Becker, 1964; Mincer and Polachek, 1974; Mincer and Ofek, 1982). A complementary argument is that women opt for less demanding and more flexible jobs once they have children (Killingsworth, 1987). Rather than discrimination, it is productivity differences between women and men, plus differences in non-financial job benefits, that are viewed as the primary reasons for gender differences in pay.

The second part of the chapter surveys a more diverse set of labour market theories, which incorporate forms of discriminatory behaviour in the labour market, as well as recognising the role of productivity in determining wages. Discriminatory behaviour in recruitment and wage policies might arise from straightforward prejudice (Becker, 1957). Employers may also discriminate against individual women because they are risk averse and suspect that women are more likely to leave their jobs to raise children than men. This is one form of statistical discrimination, when information about the group (women) is used as a cheap proxy for information about the individual (Phelps, 1972). Labour market discrimination may also stem from the fact that there are significant costs to finding and moving jobs for employees and that employers gain a certain amount of market power as a consequence (Robinson, 1969; Stigler, 1961; Burdett and Mortensen, 1998; Manning, 2003). In particular, employers may be able to pay their female and part-time employees less, relying on the fact that women with children are more likely to be constrained in their job mobility and location of work than men (Manning, 1996; Manning, 2003; Booth et al., 2003).

The final part of the chapter discusses institutionalist and feminist economic theory, which share a third, broader and more radical, view of unequal pay. Both theories reject a clear distinction between productivity-related and discriminatory differences in women's and men's pay (Sawyer, 1995; Humphries and Rubery, 1995). The role of employers and institutions in organising work and training their workforce is viewed as critical to the development of employees' skills and productivity (Piore and Doeringer, 1971). For example, if employers select men, over women, for training and promotion opportunities, they contribute both to differences in the productivity and the pay of their male and female workforce. Pay itself may be used as a tool to improve and motivate performance (Akerlof and Yellen, 1986), whilst training may be used as a device to protect job status and exclude other workers (Sawyer, 1995). The flip side of this is that employers in low-paying, often female-dominated, sectors may find it profitable to pay low wages and to provide very little training, accepting the cost of this strategy in the form lower workforce productivity and higher rates of turnover (Sawyer, 1995). Within these theoretical frameworks, there is more recognition of sociological and psychological behaviour in shaping wage structures, through social networks and discriminatory work cultures and through socialized differences in women's and men's competitive behaviour (Babcock and Laschever, 2003).

The idea of circular, cumulative disadvantage (Myrdal, 1944) is of particular interest and relevance to the historical and life-cycle analysis of unequal pay. This is the idea that individuals' preferences, prejudices and actions adapt to, as well as shape, their economic circumstances. The idea of adaptive social norms is also important in feminist theory (Folbre, 1994). For example, the different aspirations that parents have for their sons and daughters, the aspirations of young people themselves and decisions about education and employment may be affected by discriminatory social norms. Gender prejudice may in this way induce future gaps in education and employment, reinforcing prejudiced views. Alternatively, a positive cycle of decreasing inequality may cause outdated views to be challenged and reshaped.

2.1 Theories of non-discriminatory gender differences in pay

Neo-classical labour market theory predicts that individuals are paid a wage that reflects their economic value to their employer i.e. their marginal product. Individuals with equal skills should, it is argued, be paid the same competitive wage in a market economy. Conversely, differences in wages between individuals and groups are viewed as the outcomes of differences in their productivity. Under this view, differences in pay between women and men have been traditionally attributed to the differences in their productivity, arising from biological and social differences. The most fully developed explanations, within the neo-classical tradition, come from human capital theory and the concept of compensating differentials.

2.1.1 Human capital theory

The most comprehensive interpretation of how non-discriminatory differences in the pay of women and men arise is provided by human capital theory. The term human capital refers to the personal attributes and acquired skills that affect an individual's efficiency and performance in their job. Individuals develop their human capital through time invested, over the course of their lives, in education, health, job training and experience. The use of the term in the neo-classical economics literature dates back to the work of Chicago School in the late 1950s and 1960s (see Mincer, 1958; Becker, 1964). Human capital theory has been applied to the question of life-cycle earnings (Ben-Porath, 1967) and to the specific analysis of women's pay (Mincer and Polachek, 1974; Mincer and Ofek, 1982). The human capital framework has also been extended to analyse the gender division of work and effort within the household (Becker, 1985). Within this framework, women's and men's individual decisions about how to allocate and divide up work between work and family life are viewed as rational and complementary.

The human capital model also provides the following account of how individuals' wages change over the life-cycle (Becker, 1964; Ben-Porath, 1967; Mincer and Ofek, 1982). At younger ages, opportunities for direct financial returns to existing skills, in the form of higher wages, may be foregone in favour of further education or on-the-job training. In middle life, the financial gains from previous investments are realised in the form of higher wages. At older ages, investment in training declines since there are fewer years of life remaining in which to reap the future benefits of current investments. Over the life-cycle, wages rise, level off and finally decline as the process of ageing affects job efficiency, giving a concave life-cycle wage profile. Variation across individuals in life-cycle patterns of wage growth reflects differences in life-cycle patterns of investment in, and depreciation of, human capital. In more highly-skilled jobs and occupations, it is argued, the investment period is longer at the start, the rewards are higher and the depreciation rate is slower (Mincer, 1958).

Women's life-time participation in paid work tends to be shorter and less continuous than men's. The majority of women take periods out of paid work to raise children, although the length of time has been decreasing. Mincer and Polachek (1974) discuss the implications this has for women's patterns of human capital investments. First, they argue that women will make fewer initial investments through training or other learning activities in their work, since there are fewer anticipated years of employment in which to reap the rewards of those investments. Second, they argue that women's life-time investment profiles will be different to men's, since they are likely to have several separate periods of employment, rather than one continuous period.[1] Third, they argue that women who do not marry or have children are likely to have similar, but nevertheless lower, investment profiles than men. The argument here is that, at younger ages, these women may have anticipated getting married or having children and would have adjusted their investment behaviour accordingly.

Periods of absence from the labour market are argued to have a doubly-negative impact on human capital. First, they represent a period of foregone investment. Second, they lead to depreciation of existing human capital (Mincer and Polachek, 1974). Depreciation here refers to the process whereby job skills are lost or become outdated through non-use. The authors speculate further that depreciation rates are greatest

[1]They argue that a stronger expectation of prospective employment continuity once children reach school-age will provide a resumed incentive for women to invest in job-related human capital.

for on-the-job accumulated components of human capital, especially firm-specific skills. This would mean that those jobs requiring the highest levels of training would be those with the highest rates of depreciation. Polachek (1981) developed this aspect of the theory, arguing that women are likely to choose jobs and occupations in which job skills decrease at a lower rate when not continuously used.

Closely linked to theoretical work on human capital is the development of neo-classical economic models of family behaviour (Becker, 1991). These models emphasise the role of individual, rational choice in deciding upon the household division of labour. Becker (1991) has argued that the large advantages to be gained from specialisation within a family can mean that small initial biological or cultural differences between women and men in childrearing skills could result in the more marked, existing gender division of labour.

2.1.2 Compensating wage differentials

Considering the value of a job from the standpoint of employees, a further, complementary, explanation offered for women's lower rates of pay is that women receive relatively better non-wage benefits from their employers than men (Killingsworth, 1987). The idea behind compensating wage differentials is that we should think of job attributes as a whole, including wages, non-wage benefits and working conditions. From this perspective, wages vary across jobs, in part, to offset the non-pecuniary benefits and costs. Women may be more likely than men to require flexibility from their employer with respect to working hours, times of work and the location of work. In return for these non-wage benefits, which impose costs on employers, women accept lower rates of pay.

2.2 Models that incorporate labour market discrimination

The starting point for theoretical and empirical work on discrimination is that there are systematic differences in the pay of women and men not accounted for by differences in their human capital. Pay discrimination is conventionally defined by economists as a difference in rates of pay for individuals with the same productive characteristics (human capital), based on a group characteristic generally treated as being beyond individual control, such as gender or race. Theories of discrimination have to deal with the problem of how equally-productive individuals might end up being paid a wage-rate less or more than their economic value (marginal product). Three models of labour market discrimination are discussed in this part of the chapter.

Although the three theories incorporating labour market discrimination are grouped together here, there are important distinctions between them. Becker (1957) proposed a theory of discrimination based on the idea of discrimination as non-competitive behaviour, associated with pre-existing social prejudice, in an otherwise competitive labour market. In contrast, the two other theories included in this section focus upon the economic powers and motives which employers have to discriminate between groups, placing discriminatory behaviour within the broader context of an imperfectly competitive labour market.[2]

2.2.1 The economic effects of discriminatory preferences

The role of social custom and convention in constraining the terms upon which women participate in the labour market has a long history in economic thinking (see responses to the Royal Commission on Equal Pay, 1946). However, social custom and convention are viewed in quite a different way in different economic approaches to the labour market. This section focuses on Becker's theory of discrimination, in which the causes of discriminatory 'tastes' are taken as given, determined outside the realm of economic behaviour, whilst the consequences of these discriminatory tastes in a competitive labour market form the subject of economic analysis.[3]

In his book on labour market discrimination written in 1957, Becker stated his intention of supplementing 'the psychologists' and sociologists' analysis of causes with an analysis of economic consequences' (Becker, 1957, p.11). His starting point is that employers, employees and consumers have given preferences and prejudices for employing, working with or buying from one group rather than another.[4] He then models this 'taste for discrimination' as the willingness to pay a certain amount to have contact with or to avoid a particular group. The price which individuals are implicitly willing to pay to transact (or avoid contact) with one group rather than another is a key concept in Becker's analysis, which he terms the 'discrimination coefficient'. To measure the overall effect of discrimination on wages, Becker defines the 'market discrimination coefficient'

[2]This distinction is of empirical importance when it comes to quantifying discrimination, since it affects the selection of individual characteristics that are taken to be reasonable measures of productivity, and thus the size of the unexplained element of the gender pay gap that is attributed to discrimination. This is discussed further in chapter 5.

[3]Alternative economic perspectives view the relationship between social norms and economic behaviour as dynamic and endogenous. These are considered in the final part of the chapter.

[4]Becker applies this model to race and religion, as well as gender.

as the difference in average wage rate between the discriminated and non-discriminated groups, divided by the average wage rate for the discriminated group.

Becker (1957) demonstrated how variation in discriminatory behaviour across different employers could lead to segregation of discriminated and non-discriminated groups into different employment sectors, as well as to wage differences. He analysed the effects of employer, employee and consumer discrimination separately and considered their combined effects.

Becker's model assumes full employment. Theoretical work building on Becker's model has shown how the method can be extended to the analysis of differences in unemployment between discriminated and non-discriminated groups (Sowell, 1981). The effects of the discriminatory wage rate on the labour supply of the discriminated group implies that the market discrimination coefficient derived by Becker understates the full extent of discrimination.[5]

Becker's analysis suggests that competitive pressures should reduce and eventually eliminate discrimination. Discriminatory employers are paying a price for their behaviour and should eventually either change their practices or be driven out of the market by more profitable, non-discriminatory employers. Further, competition in capital markets should penalise the discriminatory behaviour of private monopolies. This aspect of his work has attracted criticism. The fact that wage differences persist between different ethnic groups and between women and men in market economies, and are not fully accounted for by differences in human capital investment is taken as evidence of persistent discrimination (Lundahl and Wadensjö, 1984). Exploring this issue, Akerlof (1980) has explained the tenacity of discriminatory behaviour by developing the idea that there are economic penalties associating with breaking the rules.

A further criticism of Becker's theory is that it does not question why people hold prejudiced views (Lundahl and Wadensjö, 1984). Becker refers to the degree of contact or non-contact between groups and the effect of this on tastes for discrimination, but he views the origins of these tastes as mainly a question for psychology or sociology. This contrasts strongly with feminist and institutional perspectives on discrimination, in which social custom is viewed as not just a determinant of economic behaviour, but as something which is shaped by economic circumstances and institutional structures.

[5] This is an important result, to which we will return.

2.2.2 Risk aversion, stereotyping and discrimination

If an employer believes that women are generally less productive than men, based on past experience or on stereotypes, then he or she may base their recruitment decisions partly on an applicant's sex, using this as a cheap proxy for missing information about an individual applicant's likely productivity and long-term job commitment (Phelps, 1972).[6] Another scenario is that the variation in productivity may be greater amongst women than amongst men. If employers are risk averse, they may discriminate against women as the group with the most uncertain productivity.[7] In practice, a common reason given by employers for not employing or promoting women is that women are more likely than men to leave jobs, permanently or temporarily, to raise children. Ruhm (1998) includes a discussion of the economic effects of parental leave policies in a labour market with asymmetric information.

2.2.3 Monopsonistic models of discrimination

The monopsony model of the labour markets starts from the idea that there are significant costs to individuals involved in finding a job and in moving job. This means that the labour supply to an individual employer is not perfectly elastic. In other words, workers will not normally leave their job in response to a small change in their pay or hours of work (Manning, 2003). Also, people who are unemployed will not search indefinitely and may accept a job that pays less than their potential economic value to an employer. Unlike the competitive market model, employers are not price takers and are assumed to have a degree of flexibility in wage-setting. Rather than a single, equilibrium wage for employees of the same skill, the result of this is a distribution of wages, reflecting different employers' trade-offs between wage levels, workforce quality and turnover.[8]

A dynamic job search model originated in the work of Robinson (1969) on monop-

[6]The role of information and uncertainty in the labour market has been developed in a more general context by Spence (1974). He proposed a theory of signaling, in which potential employees use education to send positive signals about their productivity to potential employers. Signaling is a deliberate strategy; in contrast to information conveyed by group affiliation such as gender or race, over which individuals have no control.

[7]The self-fulfilling and reinforcing aspects of this behaviour form the starting-point for more radical theories of discrimination such as Gunnar Myrdal's analysis of race discrimination in the United States (Myrdal, 1944). This point is expanded in the final part of the chapter.

[8]Another theory based on labour market frictions, which predicts some similar outcomes, is insider-outsider theory (see Lindbeck and Snower, 2001). Individuals in employment (insiders) enjoy more favourable employment opportunities than unemployed individuals (outsiders) owing to the costs of labour turnover to employers.

sony and of Stigler (1961) on the importance of frictions related to the costs of search. The dynamic behaviour of a labour market with frictions has been further developed by Burdett (1978), Burdett and Mortensen (1998) and Manning (2003). From an employee's perspective, there will be a wage benefit from spending time and energy searching for another job, either through obtaining outside offers as a means of increasing their wages in an existing job, or through moving to a better-paid job.[9] Manning (2003) argues that job search models provide an alternative interpretation of the observed experience-earnings profile (see chapter 6). Rather than being a function of human capital investment, the increase in earnings with experience can be interpreted as a function of the number of job offers received by a worker over the course of their working lives, with the highest rates of job search, job mobility and wage growth likely to occur at younger ages.

The monopsony/job search models provide an alternative explanation for sex discrimination in the labour market. In a monopsonistic labour market, women may be paid less than men, owing to family-related constraints upon their mobility between jobs and to constraints on the time and distance they are able to commute to work (see [chapter 7] Manning, 2003).[10] Employers may also use gender or part-time status as a way of identifying employees who are less likely to move job and who are more likely to accept a lower wage in their current job (Booth et al., 2003).[11] In contrast with the human capital interpretation, the negative impact of caring responsibilities on pay is, at least in part, through constraints on job mobility and bargaining power, rather than through negative effects on productivity.

The implications of the search model for labour supply motivated the earliest work on selectivity bias in wage comparisons (Gronau, 1974). Gronau (1974) pointed out that the dispersion of wage offers, resulting from the combination of employees' search strategies and employers' wage-setting and recruitment strategies, implied that the observed wage distribution could represent just that part of an underlying wage offer distribution. Wage offers considered too low to be acceptable would not be accepted or observed. More generally, the analysis of unemployment becomes more complex when the costs of job search and the resulting dispersion of wage offers are taken into account (see Manning (2003), chapter 9). The important point here is that monopsonistic

[9] Investment in job search is distinct from human capital investment since it yields no improvements in an individual's productivity.

[10] Manning (2003) also shows how job moves less motivated by money would make a difference within this framework. This is close to a compensating differentials interpretation of gender differentials.

[11] Manning (2003) points out that the lower elasticity of women's, compared to men's, supply to a particular employer is distinct from their relatively higher elasticity to the labour market as a whole.

models of discrimination predict the potential underemployment, as well as the potential undervaluation, of women's labour.

2.3 Institutional and feminist perspectives on unequal pay

Approaching the problem of unequal pay from a third and more radical starting point, institutional and feminist perspectives share a sceptical view of the distinction between productivity-related and discriminatory differences in pay. Institutional theory emphasises the role of employers in the organisation of training, work and pay, simultaneously affecting the pay and the productivity of their workforce. From this perspective, gender discrimination in labour markets is viewed as affecting the development of women's and men's human capital, as well as its relative valuation. Feminist theories view pay discrimination and inequality from the even broader perspective of how society divides up and values different forms of paid and unpaid work. Both theories emphasise the dynamic and cumulative dimensions of inequality and the feedback effects between economic and social behaviour.

2.3.1 Institutional approaches to the labour market

Institutional perspectives contrast with the models of labour market discrimination considered earlier on in the chapter. From an institutionalist point of view, gender discrimination does not just involve women being paid less than men relative to their economic value, but encompasses unequal access to employment, training and promotion opportunities which in turn result in productivity differences, with cumulative impacts on unequal pay.

Early debates on equal pay amongst economists and social reformers in Britain emphasised institutional and cultural influences on both productivity and pay. Presenting his analysis of wage differences between women and men in the Economic Journal in 1891, Sidney Webb concluded that social custom was a major factor holding back women's relative pay, and that an increase in the education and trade union membership of women would improve their productivity as well as their pay:

> Summarizing roughly these suggestions, it may be said that women's inferiority of remuneration for equivalent work is, where it exists, the direct or indirect result, to a very large extent, of their past subjection; and that, dependent as it now mainly is upon the influence of custom and public opinion,

> it might be largely removed by education and combination among women themselves. I am inclined to hope most from a gradual spread of trade unions among women workers; and that even more in the direction of an increase in the efficiency of labour which trade unionism so often promotes, than in the improvement in its remuneration arising merely from collective bargaining. (Webb, 1891, p.661)

In her submission to the Royal Commission on Equal Pay (1946), Barbara Wootton similarly argued that pay discrimination should be viewed in a larger perspective than the question of the relative efficiency and pay of women and men in the same jobs. She thought that prejudice and convention would be weakened by the very fact of legally enforcing equal pay for equal work, and that this represented a positive and necessary first step toward an improvement in women's position. She viewed social factors as central in determining employment and wages:

> I think we get nearest to the truth of the matter if we simply say that both the relatively low level of women's wages, and the relatively restricted scope of their employment, are merely reflections of the general position of women in contemporary society (Appendix IX, p.114).

A concept of circular, cumulative causation was proposed by the Swedish economist Gunnar Myrdal in his analysis of race discrimination in the United States at a similar time. In his book, *An American Dilemma*, written in 1944, he argued that prejudices and discriminatory behaviour of white people, the resulting low standards of living and health amongst black people, and their poorer ambitions and education were mutually and negatively reinforcing. He did not limit himself to purely economic factors and he argued that action on all fronts was necessary to tackle discrimination (Myrdal, 1944).[12]

The development of an institutional model of the labour market in the 1970s was stimulated by perceived flaws with the human capital theory of training. The human capital model assumes that workers will pay for skills which are transferable between employers and will reap the rewards of these investments. In turn, employers will pay for firm-specific training (Oi, 1962; Becker, 1964). Piore and Doeringer (1971) argued that this analysis of training omitted the important influence of the training process itself and the institutional environment in which training took place. From this starting

[12]Alva and Gunnar Myrdal also together wrote on the questions of population, gender equality and social welfare, influencing the Swedish model, which has a strong emphasis on childcare.

point, they developed their concept of an internal labour market. They argued that on-the-job training contributed to a large proportion of the skills actually used in a job and was also a prerequisite for the successful utilisation of formal education. They further observed that the importance of specific training increased the costs of turnover for an employer, particularly for highly-skilled jobs, and thus encouraged use of long-term contracts in employment sectors requiring skilled employees. As a consequence of this, Piore and Doeringer (1971) argued that, within internal labour markets, the relationship between wages, training and productivity is indeterminate, writing:

> Neither Becker nor Oi emphasised the startling implications for neoclassical wage theory of the permanent employment relationship. As noted, when the relationship is permanent, neither employers nor workers necessarily concern themselves with the connection between wages and marginal productivity at any point in time. These is instead a much more liberal, but nonetheless competitive, constraint that relates labour costs, earnings and productivity streams over the employee's entire work career within the enterprise. As a result, there is a set of internal wage structures consistent with this constraint (p.76).

Acemoglu and Pischke (1999) present an interesting analysis of how policies which compress wage structures, e.g. minimum wage policies, can actually stimulate firm investment in training. Within the empirical literature, a number of studies have focused on gender discrimination within particular sectors and specialist job markets. For example, studies for the UK have focused on differences in both promotion and pay within academic science (Connolly and Long, 2007) and in economics (Blackaby et al., 2005).

Piore and Doeringer (1971) also developed the concept of a segmented or dual labour market, identifying this as the consequence of the need to train and retain one part of the workforce, whilst another part of the workforce remained in a secondary sector of unprotected and less-skilled jobs paying the competitive, market-clearing rate.[13] They argued that it may be hard to escape the secondary sector once working in it, due to lack of opportunities to develop primary sector skills. Sawyer (1995) has discussed low pay in itself as a cause or contributory factor to low productivity, affecting the health, strength and commitment of workers, the organisation of work and the amount of training they receive. This is similar to Myrdal's (1944) idea of circular disadvantage.

[13]In unprotected jobs, provided there is a sufficiently large pool of unskilled or disadvantaged workers, employers may find it profitable to pay low or minimum rates and permit high levels of turnover.

A final aspect of the institutionalist analysis is the emphasis upon the role of social custom in shaping firms' internal wage structures (Piore and Doeringer, 1971). A related literature has looked at the role of economic incentives within firms, captured in the idea of the efficiency wage (Akerlof and Yellen, 1986). In contrast to neo-classical wage models in which wage-rates reflect productivity, the productivity of workers is also viewed as a function of the wage paid. The efficiency wage model predicts that higher wages will improve productivity by reducing turnover and/or inducing training or effort.

2.3.2 Feminist perspectives

Feminist perspectives view unequal actions and outcomes in the labour market, including pay, as part of a wider unequal division of paid and unpaid work in society.

The distribution of work and costs involved in caring for children and other dependants is viewed as critical. This is viewed as affecting the valuation of women's market work, not just the distribution of household work (Himmelweit, 1999). Certain occupations may be perceived to be non-skilled sectors simply because they involve types of work mainly done by women. Grimshaw and Rubery (1995) note that certain sectors, such as residential care, which display many features of secondary sector employment such as low pay, high turnover and poor training, could easily be highly-skilled and highly-valued. They argue that it is owing to the fact that care work is associated with the free labour of women, rather than for any other reason that pay, training and conditions are so poor.

Feminist economists also take seriously the idea that preferences are not exogenous but are instead are adaptive to expectations, which in turn depend upon economic and social opportunities. Social norms are viewed as particularly important in understanding the persistence of gender inequalities in power and in understanding why caring work is so poorly valued in society and why differences in women's and men's pay persist. The neo-classical idea that behaviour is purely rational and self-interested is rejected. The idea of an autonomous individual who can act, develop tastes and hold preferences independently of his or her environment is also questioned. Folbre (1994) argues that the distribution of assets, together with rules, norms and preferences give power to certain social groups, including more power to men than to women. She collectively terms these the 'structures of constraint', which she argues put boundaries upon individual choice. Folbre (1994) also questions how individuals define their self-interest, and suggests that sometimes loyalties may be divided and individual self-interest may not be straightforward. Nussbaum (2001) has further explored the idea of adaptive preferences and the idea that people in poor or difficult circumstances, and particularly women, will adapt their aspirations and expectations in line with their circumstances.

Babcock and Laschever (2003) have analysed the way that behavioural differences between women and men in the workplace can lead to gender gaps in pay and promotions. Their argument is that women are less likely than men to negotiate and ask for what they want owing to powerful social forces in modern society which discourage them from doing so (p.12). Similar to Nussbaum (2001), the adaptive behaviour of women to constraining social norms is viewed as a social problem, on the basis that 'fairness as a principle doesn't work if applied only in response to demand; it must be safe-guarded and promoted even when its beneficiaries don't realise what they are missing' (Babcock and Laschever, 2003, p.55).

2.4 Conclusions

The theories reviewed in this chapter lend themselves to three alternative explanations for gender differences in pay:

1. Differences in women's and men's parental responsibilities lead to differences in employment choices, productivity and pay;
2. Gender discrimination in the labour market, as well as differences in productivity, lead to differences in pay; and
3. Unequal treatment of women and men in work leads to gender differences both in productivity and in pay.

The rest of the thesis focuses on two measures of unequal pay derived from the last two of these perspectives, focusing on these in reverse order. Chapters 3 and 5 focus on a broad societal measure of unequal pay implied by an institutionalist/feminist perspective i.e. that unequal pay should be measured as the whole gender gap in pay opportunities. Chapters 6 and 7 focus on a narrower measure of unequal pay implied by labour market models that incorporate discrimination i.e. that unequal pay should be measured as the difference in pay between equally-qualified women and men.

The next chapter focuses on the methodological problem of estimating pay opportunities, when pay is only observed for individuals who are in employment. The motivation for this is that employment decisions are themselves likely to be contingent on potential earnings. As a consequence, unequal pay opportunities for women imply not just lower relative pay, but also lower rates of employment compared to men. For this reason, a broad measure of unequal pay should take into account the pay opportunities of those not in work.

Chapter 3

Methodological literature I: Measuring pay opportunities

This chapter examines the problem of estimating pay opportunities, taking into account the potential pay of non-employed individuals. The motivation for this is that what we really want to know about is women's and men's underlying pay opportunities, not just their pay contingent on employment decisions that are in turn affected by what they can earn in work.[1]

A standard way to think about the difference between the average observed pay of employees and the average underlying potential pay of a whole population or group is as a problem of selectivity bias, since the difference is induced by non-random selection into employment. Alternative statistical methods for dealing with selectivity bias are reviewed in this chapter, forming the basis for the empirical analysis of cross and within-cohort trends in pay opportunities presented in chapter 5.

This first part of this chapter sets out the problem of selectivity bias in wage comparisons formally and in more detail. The second part of the chapter reviews the different statistical methods proposed in the literature for accounting for selectivity bias. The third part discusses the existing empirical evidence on trends in women's and men's relative pay opportunities for the UK, Europe and the US.

[1] In this chapter, I have used the term 'potential wage' to mean the wage that a non-employed individual could earn if they entered work. In the economic literature, the terms 'wage offer' has also been used.

3.1 Selectivity bias in wage comparisons

A framework for thinking about the relationship between employment decisions and potential wages includes three sets of factors.

- First, there are factors that affect the value placed on time at home that are not related to the potential wage, such as the value placed on childcare and housework and the amount of other sources of household income.[2] These factors combine to determine the 'reservation wage' i.e. the wage rate at which an individual is willing to take up paid employment.
- Second, individual productive characteristics, such as education, determine the value of time at work, affecting the potential wage, and also, possibly, the value of time at home (the reservation wage).
- Third, structural factors, such as employer discrimination and job search intensity, may differentially affect the value of the potential wage for different individuals and groups.

The observed wage distribution can be thought of as one part of an underlying wage-offer distribution for a given population or group. For employed individuals, the potential wage is taken to be the wage that they are currently earning, though, from a search-theoretic perspective, this could be seen as just one in a range of potential wages that they could accept, depending upon their job search activity. For non-employed individuals, the potential wage is defined as the wage (or one in a set of wages) that they could earn if they entered paid work. The expected potential wage can be written out as a weighted average of the expected potential wage for employees (observed) and for non-employees (unobserved):

$$E(w^o) = \underbrace{E(w^o|s=1)}_{\text{observed}}.P(s=1) + \underbrace{E(w^o|s=0)}_{\text{unobserved}}.[1-P(s=1)] \tag{3.1}$$

where w^o is the individual potential wage and s represents employment status (1 = employed, 0 = not employed). For the purposes of setting out general assumptions and relationships, the conditional expectation $E(.)$ is thought of here as the limiting mean of an infinitely large, hypothetical, sample of individuals. From equation 3.1, it

[2]For the purposes of the analysis here, it is not necessary to distinguish between the objective value of the work done in the household, the individual preference for this kind of work or the institutional and financial constraints which shape the costs and benefits of alternative employment strategies.

becomes clear that the size of the difference between the expected potential wage for the whole population, $E(w^o)$, and the expected actual wage for employees, $E(w^o|s = 1)$, is affected both by the expected employment rate, given by $P(s = 1)$, and by the expected wage offer for non-employees, $E(w^o|s = 0)$. The difference between $E(w^o|s = 1)$ and $E(w^o)$ is the selectivity bias.

The employment rate can be thought of as being jointly determined by the wage-offer distribution and the distribution of reservation wages (at which individuals will accept work). Generally, it is assumed that

$$P(s = 1) > \frac{1}{2} \iff w^o > w^* \tag{3.2}$$

where $P(s = 1)$ is the probability of being in work, w^o is the potential wage and w^* is the reservation wage.

Concern about selectivity bias in wage comparisons, associated with employment non-participation, was raised in the econometric literature more than thirty years ago (Gronau, 1974; Heckman, 1977; Heckman, 1979). Gronau (1974) had in mind a monopsonistic/search model of the labour market, in which an individual faces a distribution of wage offers, rather than a single, competitive rate for their labour. His concern was that the observed wage distributions for groups characterised by partial employment participation represented just the better, acceptable part of a broader underlying distribution of potential wages faced by the whole group, including the non-participants.

As a consequence of selectivity bias, he argued that wage comparisons across groups with different rates of employment, such as women and men, gave a misleading picture of group differences in underlying wage offers.[3] This is illustrated in figure 3.1. The figure shows the wage-offer distributions for women and men, simplified so that all women have a fixed, common reservation wage, w^*f, and all men have a fixed, common reservation wage, w^*m. Together with the wage-offer distribution, the reservation wage determines the employment rate for each gender. The average potential wage for all women, w^of, is substantially lower than the average wage for employed women, wf, owing to a significant amount of non-employment induced by the relatively high position of the female reservation wage (w^*f) in the wage-offer distribution. For men, the difference between w^om and wm is smaller, since the male reservation wage w^*m comes low down in the male wage-offer distribution. As a consequence, the difference in the average

[3]At the time Gronau was writing, the US Census showed that around half of women were in paid work, compared to 95 per cent of working-age men (Gronau, 1974, p.1127)

observed wage for employed women and men, $wm - wf$, is smaller than the difference in average potential wages, $w^o m - w^o f$.

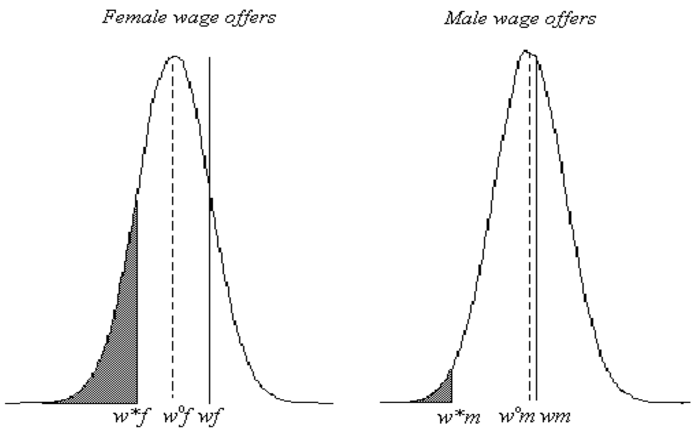

Figure 3.1: Difference in stylised wage-offer distributions for women and men

Gronau (1974) also analysed the effects of a shift in the wage-offer distribution over time. He pointed out that the effect of an improvement in potential wages would be an increase in rates of employment participation and in average pay. He noted, though, that the increase in average pay would lag behind the improvement in the average underlying potential wage. This is illustrated in figure 3.2, which shows the effects of a shift in the wage-offer distribution across two time periods, where the reservation wage, w^* is fixed at a potential wage below which individuals do not work. The problem is simplified to one where everyone has the same reservation wage at both time periods. w^o1 is the average potential wage at time period 1 and $w1$ is the average observed wage. At time period 2, the wage-offer distribution has shifted to the right, resulting in a smaller fraction of rejected potential wages (the shaded area below w^*), representing a higher employment rate. The increase in the average wage, $w2-w1$, is smaller than the increase in the average potential wage, $w^o2 - w^o1$. This scenario could be particularly relevant when analysing cross-generational improvements in women's relative pay opportunities, given the simultaneous increase in their rates of employment.

Gronau also considered a third, more complicated example of the distorting effects of employment selection on estimated age-wage profiles for women. This arises from life-cycle changes in the value of women's time at home (the reservation wage). He discussed a specific scenario in which the effects of new mothers' withdrawal from the labour market would mask the decline in their potential wages:

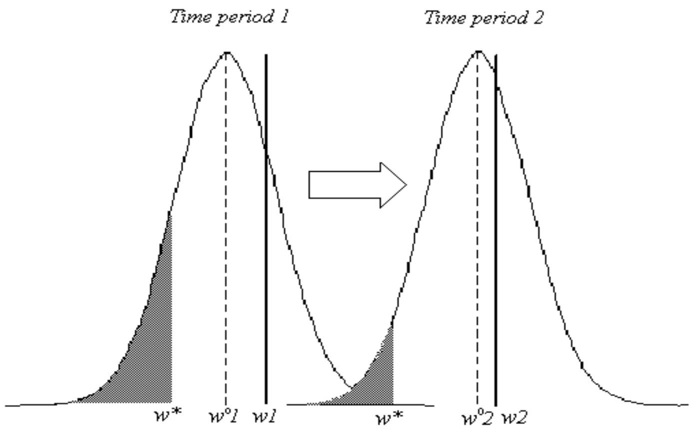

Figure 3.2: Change in stylised wage-offer distribution across two time periods

> The existence of young children increases the demand for the wife's time at home and the price of her time and reduces her tendency to participate in the labour force. Leaving the labour market may accelerate the depreciation of the woman's market-oriented skills and shift her wage-offer distribution downward. However, the decline in her wage-offer distribution does not necessarily result in a decline in the observed wage of working mothers. If the increase in these women's wage demands is sufficiently large, it will offset the decline in potential wages and result in an increase in the observed average wage (Gronau, 1974, p.1129).

Lewis (1974), and most subsequent applied work on selectivity biases in gender wage comparisons (e.g. Bloom and Killingsworth, 1982; Zabalza and Arrufat, 1985), approach selectivity bias in terms of unobserved individual differences between participants and non-participants. This is quite different from Gronau's search-based model and is rooted in a neo-classical view of wage-setting in the labour market. In the neo-classical model, an individual faces a single wage offer, not a range of potential wages. From this perspective, two observably-identical individuals, one in employment and one not in employment, necessarily differ either in the value of their time at home (their reservation wage) or in their potential wage. The standard assumption is that differences in the potential wage across individuals reflect differences in unobserved market characteristics. An alternative possibility, which is discussed in chapter 6, is that discriminatory treatment may differentially affect potential wages for different groups

(Neuman and Oaxaca (2004)).

3.2 Alternative methods for estimating potential wages

Our aim is to estimate the expected potential wage for the whole population, including non-employees, $E(w^o)$.[4] However, we do not observe potential wages for non-employees. The problem is one of making reasonable, informed estimates of potential wages for non-employees. It is possible to do this if we are willing to analyse our observed data using a number of additional modeling assumptions.

The statistical methods reviewed here draw on one of three assumptions in order to estimate potential wages:

1. we assume that all the important determinants of the potential wage are contained in the data i.e. $E(w^o|X, s = 1) = E(w^o|X, s = 0)$ where X is a vector of determinants;

2. we assume that partial information on the determinants of the potential wage are contained in the data and are also willing to assume positive selection into work i.e. $E(w^o|X, s = 1) > E(w^o|X, s = 0)$; or

3. we assume that important determinants of the wage offer are missing from the data, such that these also affect the employment decision (either directly or via the potential wage) i.e. $E(w^o|X, s = 1) \neq E(w^o|X, s = 0)$, but we have some additional, independent information on the determinants of the reservation wage (w^*).

The three selection models represented by these three alternative assumptions can be characterised as *selection on observables*, *positive selection* and *selection on unobservables*. The three alternative selection models, and the associated methods for estimating potential wages, are discussed in turn. Which assumption is used depends upon the data available, as well as on the theoretical stance that is taken on the nature of the relationship between employment, wages and other individual and work characteristics.

[4] Alternatively, we might want to estimate other moments of the wage-offer distribution.

3.2.1 Assumption 1: selection on observables

The assumption of selection on observables is equivalent to the *conditional independence assumption.* This states that, conditional on observed characteristics in X, the systematic difference in the expected wage-offer for employees and non-employees (the selection bias) disappears. This can be written:

$$E(w^o|X, s = 0) = E(w^o|X, s = 1) = E(w^o|X) \tag{3.3}$$

or

$$\{w_0^o, w_1^o\} \perp\!\!\!\perp s|X \tag{3.4}$$

where w_0^o is the potential wage if not in work ($s = 0$) and w_1^o is the potential wage if in work ($s = 1$).

Using the conditional independence assumption, the complete-case data for employees can be reweighted to directly estimate $E(w^o) = \int w^o f(w^o|X, s = 1)g(X)dX$. Alternatively, missing potential wage data for non-employees can be replaced with wage data from employees who have the same characteristics, using equation 3.3.

Several studies have used simple re-weighting approaches to analyse trends in employment and earnings (e.g. Smith and Welch, 1986; Welch, 1990; Juhn, 1992). For example, Welch (1990) used data from the US Current Population Survey (CPS), which interviewed individuals at two points a year apart over the period 1977/78-1983/84. He divided the sample into four groups based on their employment status at both surveys. He assumed that the average potential wage for non-participants who had been out of work at both surveys would be equal to the observed wage of participants who had been out of work at only one of the two surveys and reweighted the data on this basis. Juhn (1992) also implicitly assigned non-workers the wages of workers in groups with the same levels of education and employment experience, reweighting the observed wage sample to reflect the distribution of these groups in the whole population.

DiNardo et al. (1996) proposed a semi-parametric re-weighting estimator which assigns a different weight to each individual complete-case cell, rather than using groups. Another form of re-weighting estimator uses the propensity-score (here, the conditional probability of missingness) to match complete to incomplete cases and re-weight the complete-case data accordingly. Rosenbaum and Rubin (1983) have shown that conditioning on the propensity score is sufficient to remove bias arising from selection on observables, so equation 3.3 can be re-written as:

$$E(w^o|p, s = 0) = E(w^o|p, s = 1) = E(w^o|p) \tag{3.5}$$

where $p = P(s = 1|X)$.

Another approach has been to directly replace (impute) missing potential wages for non-employees with values drawn from the observed distribution of wages for employees with similar characteristics in X. This approach has come out of statistical work on methods to handle bias arising from missing data in surveys (see Rubin, 1976; Rubin, 1987; Little and Rubin, 2002) and out of methods developed by Government statistics agencies to handle missing items of data in large-scale government surveys (for example, see Sande, 1982; Skinner et al., 2002; Durrant, 2006). For each missing item of data, an attempt is made to find a donor record without missing data, most similar to the survey record with the missing item (according to some measure of distance on a set of observed characteristics) and to use the observed value to fill in the missing item. Nearest-neighbour matching, predictive mean matching and hot deck imputation based on imputation classes are some of the methods developed for identifying a 'match' for a record with a missing item of data. An example of an application of predictive mean matching to impute low pay in the Labour Force Survey can be found in Skinner et al. (2002) and Dickens and Manning (2004). Here, individuals with missing hourly wages have these replaced with values borrowed from individuals who have observed wages and who have the same predicted wage rate. Predicted rates are obtained from a linear regression of log wages on a set of observed characteristics.

Multiple imputation methods have been developed to introduce uncertainty (variance) into imputed datasets, designed to reflect the uncertainty associated with the imputation model used to fill in missing items. Instead of replacing a missing item of data with a single observed value, it is replaced with several values. This creates several imputed datasets from which the between-imputation variance can be calculated and factored into statistical analyses. Multiple values are obtained either by adding a random term to each predicted value, drawn from some specified distribution, or by drawing several values randomly from within an imputation class (see Rubin, 1987).

The goodness of conditional independence assumption depends critically on the variables in X that are conditioned upon. Hirsch and Schumacher (2004) have drawn attention to downward bias in estimates of wage differences between groups, when group status is not itself used in the imputation model as an attribute to match cases with missing wage data to donor cases.[5]

In the case of potential wages and employment decisions, there are also a number of theoretical reasons to question the conditional independence assumption.

- First, within neo-classical wage theory, unobservable factors such as individual motivation, which affect productivity, are viewed as important. These factors are likely to differ systematically across working and non-working populations (e.g. see Lewis, 1974 or Bloom and Killingsworth, 1982).
- Second, job search theories of the labour market imply that individuals with the same characteristics face a distribution of potential wages, rather than a single offer. As a consequence, the offers faced by those out of work, who are also likely to have invested less time in job search, are likely to feature in the lower, rejected part of the potential wage distribution (e.g. see Gronau, 1974).
- Third, institutional models of the labour market suggest the employers' strategies may vary, such that higher wages and longer-term employment contracts go hand-in-hand (e.g. see Sawyer, 1995).

All of these theoretical models predict unobserved differences in potential wages of working and non-working populations, although the interpretations of these differences are quite different.

3.2.2 Assumption 2: positive selection

An alternative assumption is that the potential wages of non-employees are systematically lower than those of employees with similar characteristics, so

$$E(w^o|X, s = 0) < E(w^o|X, s = 1) \tag{3.6}$$

Given equation 3.2, this implies that $\text{cov}(w^o, w^*|X) \leq 0$. In other words, positive selection into employment is induced by a potential wage is higher for some given set of characteristics in X, not because the reservation wage is systematically higher.

Blau and Beller (1992) employed this assumption to analyse wage and employment trends by race and gender in the US over the 1980s and 1990s. Using CPS data,

[5] This is demonstrated by analysing wage differences by union membership using partially imputed earnings data from the US CPS, which does not include union membership in the imputation model.

they experimented with imputing missing potential wages for non-employees that were different fractions (less than or equal to one) of those of employees with the same predicted wage, based on their education and employment experience. Analysing wage differentials using cross-sectional data from the UK Family Expenditure Survey (FES) for the period 1978-1998, Blundell et al. (2007) used the weaker assumption of positive selection into employment at the median i.e. for some X, the median potential wage for workers is higher than that for non-workers. Their X though included only gender, age, educational attainment and year, not employment experience.

Blundell et al. (2007) discussed the potential problems with the assumption of positive selection into employment, identifying three scenarios in which the potential wage might be positively partially correlated with the reservation wage i.e. $\text{cov}(w^o, w^*|X) > 0$.

- Firstly, individuals with higher earning power may also have more assets which could lead to some negative, rather than positive, selection effects i.e. those with higher earning power are able to not work. This may be particularly true for older individuals nearing retirement ages.
- Secondly, women with high earning power may be more likely to partner men with high earning power, again leading to women with higher potential earnings being able to not work.
- Thirdly, for older groups, highly-educated women are more likely to have young children, since they are more likely to have had their children later, and consequently are less likely to be in paid work.

Blundell et al. (2007) argued that their own, weaker, assumption of positive selection at the median is less likely to be undermined by these sources of negative selection than the stronger assumption of positive selection across the whole potential wage distribution. They also examined longitudinal data from the British Household Panel Study for the period 1991-2001 and found some indirect evidence to support their assumption. They regressed log wages for each panel on age and education and systematically compared the residuals (actual minus predicted wage) for continuously-employed vs intermittent workers and found that the residuals were systematically lower for intermittent workers. Mulligan and Rubinstein (2008) have argued that selectivity bias amongst women in the US was negative in the 1970s i.e. non-employed women had higher potential wages than employed women with similar levels of education, and only became positive in the mid 1980s. This finding is based, though, on a model that relies on an assumption that is arguably at least as strong as the assumption of positive selection itself. This is discussed in the next section.

3.2.3 Assumption 3: selection on unobservables

A final set of models for correcting selection bias allow for the possibility that there are systematic unobserved differences in the potential wages of observably-similar employed and non-employed populations:

$$E(w^o|X, s = 0) \neq E(w^o|X, s = 1) \tag{3.7}$$

Heckman's two-equation model

Heckman's two-equation selection model (Heckman, 1976; Heckman, 1979) is the standard method for dealing with 'selection on unobservables'. Gronau (1974) proposed a similar approach. The essence of this approach is to characterise the selectivity problem as one where the employment decision is affected by unobservable factors, which are also related to the potential wage. As a consequence, the expected value of the error term in the wage function for employees may be non-zero i.e. it is not just random 'noise'. The method aims to model and directly control for the correlation between the error term in the observed wage function and the error term in the employment function. The error term in the employment function is measured as the difference between some indicator of actual employment status and the predicted probability of being in employment. A function of this is then included in the wage function, to test for a partial correlation (indicating selectivity bias) and to control for this.

Using the general notation employed so far, the broad approach can be written as follows. The problem is reframed as

$$E(w^o|X, U, s = 0) = E(w^o|X, U, s = 1) = E(w^o|X, U) \tag{3.8}$$

where unobserved factors that affect both the potential wage and the employment decision can be represented by an index U. The employment function is then

$$P(s = 1) = F(X, U, Z) \tag{3.9}$$

where $F(.)$ is some function and Z is a vector of variables which affect the employment decision but not the potential wage (holding fixed X and U). We do not observe U. However, some measure of U can be inferred from the difference in actual employment status (which is affected by U) and the predicted probability of being in employment conditional on X and Z (which does not include U):

$$W(s^* - \hat{p}) = V(U) \tag{3.10}$$

where s^* is an indicator of actual employment status (based on some transformation of s) , $\hat{p} = P(s = 1|X, Z)$ and $W(.)$ and $V(.)$ are unspecified functions. Equation 3.10 can be substituted back into equation 3.8, giving:

$$E(w^o|X, \underbrace{W(s^* - \hat{p})}_{V(U)}, s = 0) = E(w^o|X, \underbrace{W(s^* - \hat{p})}_{V(U)}, s = 1) \tag{3.11}$$

Three important things should be noted about this approach.

1. In this model, $\hat{p}$ plays a very different role to p in the matching model (equation 3.5). In the matching model, the propensity score p stands in for X. In this model, the difference between s^* and $\hat{p}$ is exploited to proxy for U. Heckman and Navarro-Lozano (2004) have drawn attention to the confusion that has arisen over the role of the propensity score in selection, matching and instrumental variable methods. They provide a good summary of the different role of the propensity score in the different models and the different assumptions which justify its use in each context.

2. Within the employed sample, the variation in $V(U)$ comes solely from the variation in $\hat{p}$, since actual employment status is fixed ($s = 1$). If there are no excludable variables in the model (Z), the variation in $\hat{p} = P(s = 1|X)$ is just the variation in some function of X. In this case, the inference that the variation in $\hat{p}$ really does represent variation in $V(U)$ is based on rather shaky foundations (see Vella, 1998 and Puhani, 2000 for a review of studies that have simulated data to test this).

3. For the non-employed sample, $V(U)$ is inferred from actual employment status $s = 0$, in combination with predicted employment probabilities $\hat{p}$. Thus employees and non-employees who share the same $\hat{p}$ are assumed to be more different with respect to $V(U)$ than they are similar. For example, imagine one person who is in work, despite having small children and poor access to free childcare (and

therefore a low predicted probability of being in work), compared to another who is in a similar position, but who is not in work. $V(U)$ is a measure of the difference between them, designed to capture the effects of unobserved factors, such as greater motivation, having a better employer etc...

Heckman's original two-equation model relies on parametric (distributional) assumptions about the form of the selection bias and the structure of the error terms i.e. joint normality, separability and additivity. This structural model is written out in full at the end of this chapter. This type of model has been developed into a more general set of 'control function' methods, which rely less heavily on parametric assumptions but rely more strongly on exclusion restrictions (for good descriptions of these, see Heckman and Navarro-Lozano, 2004; Blundell et al., 2005).[6]

The main problem with Heckman type/control function methods is that it is hard in practice to find variables that determine the probability of work, but not the potential wage i.e. to find reasonable exclusion restrictions. Commonly used variables include one or more of the following: the income, education and/or age of a spouse or partner; household wealth; non-wage household income; housing tenure; and number and ages of children (for examples of studies using these, see Gronau, 1974; Zabalza and Arrufat, 1985; Dolton and Makepeace, 1987a; Ermisch and Wright, 1993; Joshi and Paci, 1998). For each of these variables, it is likely in most situations that they affect the decision whether or not to work quite strongly. What is open to debate is whether or not these variables are also directly partially correlated with the potential wage. The problems discussed already in this chapter in relation to the assumption of positive selectivity bias above also cast doubt on using household income, spouse's income or children as excludable variables.

Other less common exclusion restrictions have also been used in the literature, including individual attitudes to work and family (e.g. Albrecht et al., 2004) and holding a loan or mortgage on a property (Dolton et al., 2009). In the first of these, the use of attitudinal data also draws on a strong and debatable theoretical stance in which attitudes and preferences are taken as given (exogenous) rather than adaptive to circumstance (endogenous). In the second, the idea is that taking out a loan or mortgage is likely to put pressure on individuals to work. One practical objection to this exclusion restriction is that institutional arrangements in some European countries (the focus of

[6]In the parametric specification, the model is identified even if there is no exclusion restriction i.e. $X = Z$. In practice though, the results are sensitive to small mis-specifications in the model without an exclusion restriction (see Manski, 1989; Vella, 1998; Puhani, 2000.

the analysis) may require that individuals have a certain level of earnings before they are eligible for a loan or mortgage.

More credible and less theoretically-driven exclusion restrictions are those which draw on institutional arrangements or changes, such as out-of-work benefits entitlement, used by Blundell et al. (2007), which is discussed in the last part of this chapter.

Angrist and Krueger (2001) have drawn attention to the problems with using instruments that are correlated with the omitted variables and the same problems apply to excluding variables that are, in fact, correlated with the error term in the wage function. Further, Angrist and Krueger (2001) have pointed out that the bias in estimates from mis-specified models using instrumental variables can be much greater than the bias in estimates using ordinary least squares models, which rely on the assumption of selection on observables.

Other methods to account for unobserved selection

An alternative approach to characterising selectivity bias was proposed by Manski (1989). He pointed out that, although it is not possible to obtain selection-corrected point estimates without some additional identifying assumptions, it is possible to estimate upper and lower bounds on quantiles of the distribution. The underlying potential wage distribution - part observed and part missing - can in this way be characterised by its upper and lower limits on particular quantiles. The proportion of missing data arising from non-participation and the maximum and minimum values within which the missing potential wage data are constrained to lie determine the size of the bounds. A limitation of this approach is that, where a substantial proportion of the data are missing, precise estimates cannot be obtained. Manski suggested that additional assumptions, including exclusion restrictions, could be introduced to tighten the bounds.

Finally, the availability of longitudinal data on employment and wages for the same individuals over time makes more direct and simple methods possible. Imputing potential wages for non-employed individuals using data on their wages recorded in previous or subsequent waves of the panel (when they were employed) has the advantage that fixed individual unobserved, as well as observed, selectivity biases are taken into account in the imputed wage. Interestingly, Olivetti and Petrongolo (2008) got similar results when they imputed potential wages for a group of non-workers using information on their past wages and when they imputed these for the same group using the wages of observably-similar employees. The authors argued that, in this context, the evidence provides support for the assumption of selection on observables. A limitation of using past wages to impute missing current wages is that only fixed, individual sources of unobserved selectivity bias are accounted for. Changing or structural (vs. individual) sources of bias are not accounted for. For example, the past wages of a woman who has since had children may not be a good estimate of her current potential wage.

3.3 Existing estimates of gender gaps in pay opportunities

This last part of the chapter focuses on just four studies, each of which analyses the effects of selectivity bias in gender wage comparisons, induced by employment selection. Blundell et al. (2007) have analysed trends for the UK since 1975. Blau and Kahn (2006a) and Mulligan and Rubinstein (2008) looked at trends for the US since the 1970s. Olivetti and Petrongolo (2008) examined differences across OECD countries. The purpose here is twofold: first, to summarise the set of results that provide a context for the analysis presented in the following chapter; and second, to draw attention to the sensitivity of estimates to the method used.

Blundell et al. (2007) looked at trends in gender differentials in potential pay for Britain, using cross-sectional data from the Family Expenditure Survey (FES) comparing the periods 1978 and 1998. To analyse trends in gender differentials, they split the sample into four age-education groups: 25-year-olds with A-levels or lower qualifications; 25-year-olds with higher qualifications; 40-year-olds with A-levels or lower qualifications; and 40-year-olds with higher qualifications. They estimated bounds on wage distributions (see above, proposed by Manski, 1989), including exclusion restrictions to tighten these. In the first instance, using bounds without restrictions, they found that the bound estimated on the change in the gender differential in median potential wages between 1978 and 1998 was not informative. The low proportion of women in employment in the 1978 sample made this bound too wide to be able to get a precise estimate of change. Only by way of imposing restrictions on the model of positive selection into employment and partial exclusion restrictions (related to changes in the out-of-work benefits system) did the authors find evidence that the gender differential in potential pay had decreased for the 25-year-old less-qualified group, although not for the other groups. Further, using these restrictions, their results suggest that the gender differential in potential pay for this group has decreased more than the pay trends for employees would suggest. In other words, employment selection effects appear to conceal part of the improvement in women's earnings opportunities.

Blundell et al. (2007) used a partial exclusion restriction to estimate informative bounds on trends in gender differentials. They assumed that out-of-work benefits entitlement, holding fixed household composition, affects the employment decision (via the reservation wage) but is only weakly and positively correlated with an individual's potential wage. This is referred to as the monotonicity restriction in the paper. This variable was constructed using the Institute for Fiscal Studies tax and benefit model and was used previously in Blundell et al., 2003. The variation in this out-of-work benefits is not linked to variation in household composition, which is held fixed, but comes from variation in housing benefit entitlement, linked to local and individual housing costs. The authors rejected this variable as a full instrument, but argued that it is likely to be only weakly and positively correlated with the potential wage, whereas it will have much stronger direct effects on the employment decision. On this basis, it is regarded as suitable for a partial exclusion restriction. [7]

For the United States, Blau and Kahn (2006a) used longitudinal data from the Panel Survey of Income Dynamics (PSID) for the years 1979, 1989 and 1998. They were concerned with the question of whether changes in women's labour force participation had contributed to the slowing convergence in women's and men's average pay in the 1990s. They used an imputation approach to estimate potential wages for non-employed individuals. They exploited the longitudinal aspect of the dataset, replacing missing potential wages for individuals out of employment with the wage observed within a four-year window when that individual was last or next in employment. For individuals who had not been in paid work over this four-year period, the missing potential wage was placed above or below the median based on their level of education and employment experience. They found that trends for full-time employees overstated the convergence in women's and men's pay opportunities in the 1980s and slightly understated the convergence in the 1990s. Note though that this refers to the change in the raw, unadjusted pay gaps (reproduced below from table 3 of Blau and Kahn (2006b)), not to trends in adjusted gaps which the authors focus on.

Mulligan and Rubinstein (2008) also analysed changes in gender gaps in pay opportunities for the United States between 1975-79 and 1995-99. Their results are not directly comparable to those of Blau and Kahn (2006a), discussed above, since the results in Mulligan and Rubinstein (2008) are trends for full-timers, net of demographic

[7] The authors identify one source of a weak positive correlation induced by individuals with higher potential earnings living in better, more expensive housing. A second source which they do not consider is the possible correlation between wages and housing costs within local areas, below the regional level. However, this second effect may well be weak, since the evidence is that there is a bigger gap between wages and rents at the bottom of the market in high cost areas.

characteristics, not the unadjusted trends. Mulligan and Rubinstein (2008) used cross-sectional data from the Current Population Study for two time periods, 1975-79 and 1995-99. The authors used Heckman's two-step structural model for estimating selectivity bias, treating the presence of young children (aged five or under) in the household as exogenous in the labour supply equation and as excludable from the wage equation. The other variables included in the wage model are educational qualifications, potential employment experience, marital status and location of residence. They found that the sign on the selection term was large and negative for the 1975-79 sample, but large and positive for the 1995-99 sample. The authors interpreted these partial correlations as indications of unobserved selectivity bias; negative in the earlier sample and positive in the later sample. In other words, the predicted potential wages of non-working mothers in the 1975-79 sample were actually higher than the wages of otherwise-similar working mothers in the sample. The authors concluded that there has been no improvement in women's potential wages, relative to men's offers for fixed demographic characteristics, but that the increase in the relative wages of employed women is wholly attributable to women's changing patterns of labour market participation.

A problem with the model which Mulligan and Rubinstein (2008) use is that it relies heavily on the assumption that motherhood is not partially correlated with the wage offer, holding other characteristics. The idea behind this is that the partial correlation between the selection term (which is itself correlated with motherhood) and the potential wage arises from non-random selection of mothers with young children into employment. However, there are two ways in which this partial correlation might arise if this assumption were wrong. First, a positive partial correlation (found for the later 1997-99 sample) might arise from selectivity bias associated with both the timing of motherhood. This requires some further explanation. The employment and wage models used by Mulligan and Rubinstein (2008) include three potential experience categories (5-14 years, 15-24 years and 25-34 years). In effect, controls for age are included in the model. If there is positive selection into late motherhood and if older mothers return to work more quickly, we would get a positive sign on the selection term in the wage equation (discussed earlier in the chapter and by Blundell et al. (2007)). In formal terms, the selection term is directly correlated with the error term in the wage equation.

Second, a negative partial correlation (found for the earlier 1977-79 sample) may arise from the direct negative effects of motherhood upon women's wages, particularly when employment experience is imperfectly accounted for. Corresponding closely to the sample period for which Mulligan and Rubinstein (2008) found a negative sign on the selection term, Waldfogel (1998) found evidence to suggest that motherhood had direct negative impacts on women's wages in the United States, taking into account their qualifications, employment experience, age (fixed at thirty), marital status and ethnicity. She used data from the 1980 survey of the National Longitudinal Studies of Young Women (NLS-YW) and Young Men (NLS-YM). If this is the explanation for the negative partial correlation between the selection term and wage, then the higher potential wages predicted for non-working mothers, compared to working mothers, in the 1975-79 sample, are poor predictions and cast doubt on the interpretation given by Mulligan and Rubinstein (2008).

Olivetti and Petrongolo (2008) took a similar approach to that of Blau and Kahn (2006a) to estimate gender gaps in potential wages across fourteen OECD countries. Using data from the PSID and the European Community Household Panel Survey (ECHPS) in the period 1994-2001, they were able to place the missing wages of non-participants either side of the median on the basis of their most recently observed actual wage (when employed). For individuals who had not been in employment over the whole period, their potential wage (relative to the median) was estimated on the basis of their observed characteristics. They found that gender gaps in median potential wages were higher than gender gaps in median wages of employees in all countries, suggesting positive selection into employment. They found that the effects of including potential wages for non-employed individuals was small in countries with high rates of female employment - the UK, the US and most central and northern EU countries - but was substantial in southern Europe, where female rates of employment are much lower.

3.4 Discussion

This chapter has demonstrated the strong theoretical motivation for trying to model and take into account the bias in gender wage comparisons that arises from non-random selection into employment. In particular, the potential biases arising from interdependent changes in women's employment patterns and potential earnings have been demonstrated in different contexts.

The different approaches and statistical models for taking into account selectivity bias have been critically analysed and have been discussed in detail in relation to four studies of trends in women's and men's potential earnings.

On the basis of this review, the strategy used to analyse the cohort data is based on an imputation methodology. The working assumption is that of 'selection on observables'. This is not based on a strong theoretical stance about the absence of selection on unobservables, but rather on the combined consideration of the nature of the data available in the cohort studies and the problems with trying to account for unobserved selection using mis-specified control function models. The data contain a large amount of detailed information on individuals, but no administrative data that would lend itself to credible exclusion restrictions, such as information on local childcare provision.[8] Further, the evidence suggests that methods using mis-specified exclusion restrictions to account for unobserved selection bias may give estimates that are more biased than those from methods which do not attempt to account for unobserved selection bias at all (see discussion in chapter and Angrist and Krueger (2001)). Instead of trying to model and account for unobserved selection bias, some indirect evidence of unobserved selection bias is sought in the data and the implications for the estimated trends are considered.

Appendix 3: Heckman's structural two-equation model

In Heckman's structural two-equation model, the potential wage equation is written as:

$$\ln(w^o) = X'\beta_1 + u \tag{3.12}$$

where $\ln(w^o)$ is the natural log of the potential wage, X is a vector of observed wage-related characteristics, β_1 is a vector of partial linear correlations and u is an individual residual term. For the whole sample, the limiting distribution of error terms is assumed to be asymptotically normally distribution with mean zero and constant variance ($E(u) = 0$, $\text{var}(u) = \sigma$). The expected value of the potential wage is $E(w^o) = X'\beta$.

The selection equation is written:

$$s^* = X'\beta_2 + Z'\beta_3 + v \tag{3.13}$$

where $s^* \geq 0$ if an individual is in paid work ($s = 1$) and $s^* < 0$ if an individual is not in paid work ($s = 0$). X is a vector of characteristics also included in the wage model and Z is a vector of variables excluded from the wage equation, which are assumed to influence the employment decision, but not the potential wage.

[8]There is project to link and develop methods for analysing linked longitudinal and administrative datasets, see http://www.ncrm.ac.uk/about/organisation/Nodes/ADMIN/

The observed wage equation is estimated just on the selected sample who are in work:

$$E(\ln(w^o) \mid X, s^* \geq 0) = X'\beta_1 + E(u|s^* \geq 0). \tag{3.14}$$

The expected error term in the observed wage equation can be re-written as:

$$E(u \mid s^* \geq 0) = E(u \mid X'\beta_2 + Z'\beta_3 + v \geq 0) = E(u \mid v \geq -X'\beta_2 - Z'\beta_3) \tag{3.15}$$

The additional assumption in Heckman's original two-equation model is that the error term in the potential wage equation, u, and the error term in the selection equation, v, are jointly normally distributed, $\text{cov}(u, v) \sim N(0, \sigma_{uv})$. Using this assumption, the conditional expectation of the error term in the observed wage equation is:

$$E(u \mid v \geq -X'\beta_2 - Z'\beta_3) = \rho_{uv}\sigma_u\lambda \tag{3.16}$$

An estimate of an individual's sample selection score, λ, is calculated from the selection equation as the ratio of the normal probability density function to the cumulative density function: $\lambda(.) = \frac{\phi(.)}{1-\Phi(.)}$ evaluated at $\lambda(-X'\beta_2 - Z'\beta_3)$. λ is included as an additional regressor in the wage equation and the coefficient on λ is $\rho_{uv}\sigma_u$. Importantly, the larger the participation probability is, the smaller is the sample selection score, λ.

From this, we estimate:

$$E(\ln(w^o) \mid X, s = 1) = X\beta_1 + \rho_{uv}\sigma_u\lambda \tag{3.17}$$

and can calculate $E(\ln(w^o)|X) = E(w^o \mid X, s = 1) - \rho_{uv}\sigma_u\lambda$.

Chapter 4

Data used from the British Birth Cohort Studies

This chapter describes the data used from the three British Birth Cohorts to investigate trends in the unequal pay of women and men. The three studies are continuing national surveys of individuals born in single weeks of March 1946, March 1958 and April 1970. This chapter considers what the three cohorts, taken together, can offer to the investigation of this subject and some of the problems associated with using data from these long-running longitudinal surveys. Section 4.1 includes a general introduction to the studies and an overview of survey non-response and permanent loss of sample members over time. It also discusses the main advantages and disadvantages of using the cohort data for the present analysis. Section 4.2 describes the final datasets and derived variables used in the present analysis. It also describes the original variables used and the work done to construct the datasets. Section 4.3 includes a detailed assessment of the quality of the 1946 employment history data and of the combined effects of the survey non-response and missing data on wage estimates. The statistical treatment of these data quality issues is discussed.

4.1 Introduction to the three birth cohort studies

This section provides an overview of the study designs and changes in the studies and samples over time. The 1946 birth cohort study is housed at the MRC Unit for Lifelong Health and Ageing (see http://www.nshd.mrc.ac.uk). The 1958 and 1970 birth cohort studies are housed at the Centre for Longitudinal Studies at the Institute of Education (see http://www.cls.ioe.ac.uk) and datasets are available to the research community and can be accessed via the UK Data Archive (http://www.data-archive.ac.uk/). For a profile of each study, see Wadsworth et al. (2006) on the 1946 birth cohort study, Power and Elliott (2006) on the 1958 birth cohort study and Elliott and Shepherd (2006) on the 1970 cohort study. For a comparison of findings from the three cohorts, see the volume edited by Ferri et al. (2003).

4.1.1 Original survey designs and samples

Each of the birth cohort studies took as its original target sample all of the British births in one week. The 1946 cohort study - the MRC National Survey of Health and Development (NSHD) - started in March 1946 as a study of childbirth and maternity services, motivated by a contemporary concern about falling birth rates (Joint Committee of the Royal College of Obstetricians and Gynaecologists and the Population Investigation Committee, 1948).[1] The original study was promoted by the Royal College of Obstetricians and the Population Investigation Committee and was funded by the Nuffield Foundation and the National Birthday Trust Fund. Interviews were conducted by health visitors at home visits with all mothers who gave birth in England, Scotland and Wales in the week 3-9 March 1946. Mothers of 13,687 babies were interviewed, representing 82 per cent of the 16,695 births selected into the sample. A follow-up study was carried out in 1948, which selected a smaller, stratified sample to interview, owing to cost and technology constraints (Wadsworth, 1991). The resulting 5,362 two-year-olds has constituted the core target sample of the 1946 cohort, which has been tracked ever since. Weights are provided with the datasets to account for the stratification of the sample in 1948. A description of how the stratification weights are used is included in the final section of this chapter.

The 1958 cohort study - the National Child Development Study (NCDS) - was set up to investigate the social and obstetric factors associated with infant death and abnormalities (Butler and Bonham, 1963). In common with the original survey of the 1946 birth cohort, the original Perinatal Mortality Study (PMS) was funded by the National Birthday Trust Fund, with the administrative network of the National Health Service used to contact mothers. The study included in its sample all babies born in England, Scotland and Wales in the week 3-9 March 1958. Data were collected via a questionnaire completed by the midwife in attendance at the delivery, based on a interview with the mother, plus information extracted from medical records. Of 17,634

[1] As it turned out, this birth cohort was the beginning of a post-war baby boom.

mothers selected for the sample, 17,416 (99 per cent) were successfully interviewed. In addition, information was collected on more than 7,000 stillbirths throughout the months of March, April and May 1958.

The 1970 cohort study - the 1970 British Cohort Study (BCS70) - was also designed to investigate maternal social and biological characteristics in relation to infant health and mortality and was set up to allow comparisons with the birth survey of the 1958 cohort (Butler et al., 1986). The original survey of the 1970 cohort (the British Births Survey) was funded again by the National Birthday Trust Fund, along with the Royal College of Obstetricians and Gynaecologists, the Department of Health and Social Security and other private organisations. Data were collected via a questionnaire completed by the midwife and via medical records. The original target sample included 17,287 babies born in England, Wales, Scotland and Northern Ireland in the week 5-11 April 1970, of whom 16,571 (96%) were successfully interviewed. Subsequently, those born in Northern Ireland were not included in the study.

None of the three cohort studies follow samples that are representative of the adult population (of the same age) living in Britain, since they exclude most non British-born residents born in the sample weeks. Around 400 children living in Britain, but born outside Britain during the sample week, were recruited into the 1958 cohort study at age 7, 11 or 16. Around 300 children born outside Britain during the sample week were recruited into the 1970 cohort study at age 5 or 10. No attempts were made in adulthood to recruit new cohort members.

4.1.2 Longitudinal data collections

The first follow-up surveys of the 1946 birth cohort were carried out in 1948 and 1950 (at ages two and four) to investigate the persistence of social class differences at birth in the infant's growth and development. The 1946 cohort study became the model for the 1958 and 1970 birth cohort studies. In the 1950s and 1960s, there was an increasing interest in the research and policy community in linking children's educational, physical and mental development to their circumstances at birth. When the children of the 1958 cohort were aged seven in 1964, funding for a follow-up survey was secured from the Department of Education and Science to inform the investigations of the Plowden Committee on primary education in Britain (Department of Education and Science, 1967).[2] When the 1970 cohort children were aged three, the study transferred to the University of

[2] At this point, the study moved to the National Children's Bureau, where it remained until 1981.

Bristol, where Neville Butler organised a follow-up survey in 1975. For a discussion of the contributions of the original study directors - James Douglas, Neville Butler and Mia Kellmer Pringle - to the development and continuation of the longitudinal studies, see Bynner et al. (1998), Bynner and Goldstein (1998) and Ferri (1998).

Over time, changes in the funding and location of the studies, changes in research interests and changes in the life-stage of cohort members have contributed to changes in the types of data collected and the methods of data collection. For the 1946 cohort study, data were collected at two or three-year intervals from mothers, teachers and school medical officers in childhood and tests of general, mathematics and reading ability were administered at school. In adulthood, data have been collected through face-to-face interviews at ages 26, 36, 43 and 53 and via postal surveys at ages 20, 22, 23, 25 and 31 (Wadsworth et al., 2003). From birth to age 16, funding came from a variety of sources. Since 1960, the Medical Research Council has been the main funder and the primary focus of the study has been on health. At age sixty, cohort members have been invited to have medical examinations in clinics. For a full summary of the types of data collected and methods of data collection, see Wadsworth et al. (2006), tables 2 and 3.

For the 1958 cohort study, data were obtained at ages 7, 11 and 16 via interviews with mothers, medical records, questionnaires sent to school teachers, school medical examinations and mathematics, reading and other tests. At age 16, questionnaires completed by cohort members themselves. In adulthood, face-to-face interviews have been carried out with cohort members at ages 23, 33, 42 and 50. A telephone survey was carried out at age 46. For a full summary of the types of data collected and methods of data collection, see Power and Elliott (2006), tables 1, 2 and 3. After leaving the National Children's Bureau, the Social Statistics Research Unit at City University took over the study in 1991 and it moved again to the Centre for Longitudinal Studies at the Institute of Education in 1999/2000.

For the 1970 cohort study, data were collected at ages 5, 10 and 16 from parents, school teachers, medical officers and cohort members themselves. The Department of Health at Bristol University ran the age 5 and 10 surveys. Parents of the cohort members were interviewed by health visitors, questionnaires were sent to head and class teachers, school medical officers carried out medical examinations on each child and cohort members undertook mathematics and reading tests. The 1986 (age 16) survey was carried out by the International Centre for Child Studies. Data were collected via parental questionnaires, school class and head teacher questionnaires and medical examinations. Cohort members completed questionnaires, kept short diaries on nutrition and general activity and undertook some further tests. The 1996 follow-up (age 26) was carried out by the Social Statistics Research Unit at City University (which also then housed the 1958 cohort). A postal questionnaire was sent to cohort members for whom a current address was available. Face-to-face interviews have been conducted with cohort members at ages 30 and 34 and a telephone survey at age 38 (in 2008). For a full summary of the types of data collected and methods of data collection, see Elliott and Shepherd (2006), tables 1 and 2. Along with the 1958 cohort study, the 1970 cohort study moved to the Centre for Longitudinal Studies (CLS) in 1999/2000, with interviewing contracted out to different organisations. The Economic and Social Research Council (ESRC) have been the main funder of the 1958 and 1970 cohort studies since their move to CLS.

4.1.3 Overview of survey non-response and attrition

Cohort members are retraced for each survey and a birthday card is sent to all cohort members each year, which requests notification of any changes of name or address. Cohort members are also sent updates about research using the data they provide. Fewer resources were available to maintain contact with the 1958 cohort before 1981 (age 23) and with 1970 cohort members before 1991 (age 21). For the follow-up surveys prior to these dates, cohort members were newly traced for each survey through school registers and NHS records.

Despite work to maintain the samples, a problem common to all longitudinal studies - particularly to the long-running cohort studies - is that some individuals take part intermittently and others do not remain in the study. From a research point of view, this may lead to problems with statistical inference if either large numbers of participants leave the study and/or if those who leave are systematically very different from those who remain in the study.

The figures on non-response and attrition presented here refer only to the original British-born longitudinal sample and do not include non British-born cohort members recruited into the 1958 and 1970 cohorts in childhood. The figures shown here are taken from Wadsworth et al. (2003) for the 1946 cohort and Plewis et al. (2004) for the 1958 and 1970 cohorts. The analyses of earnings presented in the thesis were carried out using the full cross-sectional samples, including cohort members born outside Britain.[3]

[3]Between 240 and 280 non British-born cohort members remain in the 1958 cohort samples with complete data and between 130 and 160 for the 1970 cohort samples.

The main conclusions of analyses presented in this thesis were not found to be sensitive to their inclusion.

Between 11 and 13 per cent of the original samples for the 1946 and 1958 cohort studies were known to have died or emigrated by their early thirties. An estimated 6 per cent of the 1970 cohort had died or emigrated by age thirty in 2000. The lower proportion of deaths in the 1970 cohort reflects slightly lower death rates in infancy. However, mortality and permanent emigration in the 1970 cohort may be underestimated, since some untraced members, who are counted in the target sample as refusals or non-contacts, may fall into these categories. These figures are likely to be revised as more information comes to light in future tracing exercises (Plewis et al., 2004).

Figure 4.1 shows the longitudinal response rates to surveys for the three cohorts. These provide a useful indication of how the representativeness of the samples may have deteriorated over time and how this varies across surveys and across cohorts. The longitudinal reponse rate is defined as the percentage of the target samples who participated in each survey, where the target sample excludes people who are known to have either died or emigrated. In the case of the 1970 cohort, the target sample also includes a small number of untraced cases of uncertain eligibility who are included in the estimated permanent emigration category (see Plewis et al., 2004). Response rates are not shown for the 2004 survey of the 1970 cohort (at age 34) since a substantial fraction of untraced cases are of uncertain eligibility. These figures differ slightly from cross-sectional response rates and from response rates based on the sample with whom contact was actually attempted at a given survey. The target sample here includes permanent refusals, whereas the sample with whom contact is attempted excludes this group (e.g. see Wadsworth et al., 2003).

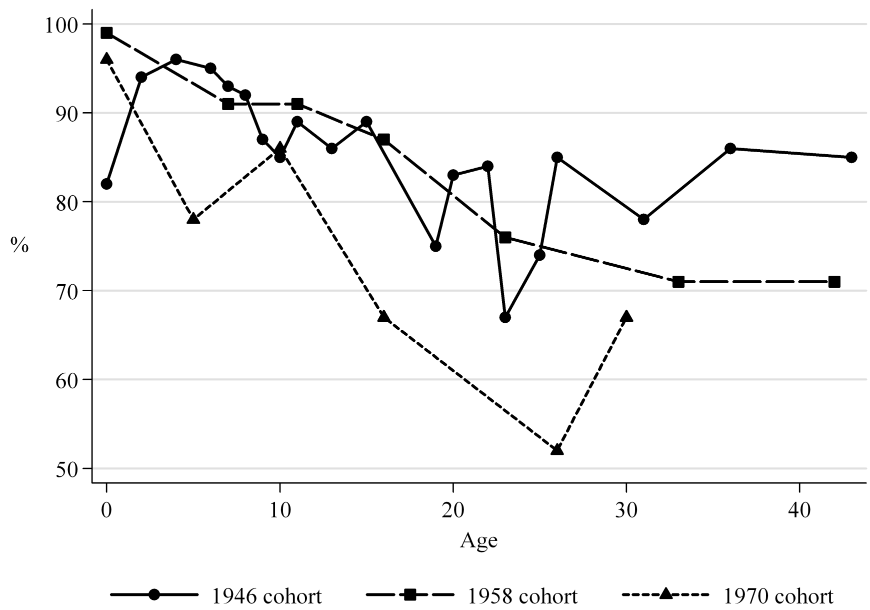

Figure 4.1: Longitudinal response rates - % of target sample (exc. deaths and emigrants), by age and cohort

A substantial proportion of cohort members who have not left the studies have participated intermittently. For the 1958 cohort, 62.5 per cent of participants in the age 42 survey had also participated in all previous surveys (Hawkes and Plewis, 2006). Response rates to postal and telephone surveys have been significantly lower than response rates to home interviews. For the 1970 cohort, response rates were particularly low to the postal survey sent out in 1996 (age 26), compared to subsequent interview-based surveys. One reason for the low response rate to this survey was that, unlike at other surveys, attempts to trace cohort members were discontinued once the questionnaire had been sent out (Plewis et al., 2004). Non-response to the age 26 survey substantially reduces the longitudinal samples used in the analysis for the 1970 cohort and is discussed further in the last part of this chapter.

Cohort members who have left the studies tend to differ systematically from those who have remained in the studies. A common finding is that there are significant sex differences in response patterns, with an under-representation of men in the 1946 and 1958 cohort studies in adulthood (Wadsworth et al. (2003), Hawkes and Plewis (2006)). Hawkes and Plewis, 2006 found that men were more likely than women to drop out of the 1958 cohort study after the age of 16.

There is also under-representation of individuals who experienced more childhood disadvantage (see Wadsworth et al., 2003, Nathan, 1999, Hawkes and Plewis, 2006). Wadsworth et al. (2003) analysed patterns of survey non-response and attrition from the 1946 Cohort Study up to the 1999 survey (age 53). They found that individuals who were shorter at age four, those who had experienced serious illnesses, bedwetting or other problems in childhood and those who had experienced material and financial disadvantage in childhood and adulthood were also more likely to have left the study. Comparing the birth and fixed characteristics of individuals who have remained in the 1958 cohort study to those of individuals who have dropped out up to age 42, Hawkes and Plewis (2006) found that the likelihood of leaving the survey was greater amongst those with lower birthweight (a proxy for social disadvantage), those born in Wales. Looking at patterns of survey non-response by time-varying as well as fixed characteristics, they also found that individuals with lower educational attainment, less stable employment patterns and more disadvantaged family circumstances were more likely to be lost from the study. Bynner et al. (1997) analysed loss from the 1970 cohort up to age 26 and found that those born to single or teenage mothers, those with unemployed fathers and with low school achievements were more likely to have left the study. There are no studies solely focused on patterns of attrition and survey non-response for the 1970 birth cohort up to age 30 or 34. Bynner and Parsons (2005) compared parental social class and educational characteristics for the birth sample to the 2004 sample; finding that individuals born to less advantaged families were slightly more likely to have left the study by age 34. They also compared characteristics of cohort members across the 2004 (age 34) and 2000 (age 30) samples, finding that the profiles were similar, although those from less privileged backgrounds were slightly more likely to have left the survey between these ages.

Despite some deterioration in the representativeness of the samples over time, both Wadsworth et al. (2003) and Nathan (1999) have concluded that serious biases do not arise from non-response for the full samples. Comparing means of key characteristics for the weighted sample of the 1989 survey (age 43) to those for a sample from the 1991 Census aged 40-44, Wadsworth et al. (2003) found evidence that the sample remained broadly nationally representative. They found that single, separated and divorced women were slightly under-represented, compared to the Census sample, and also that those in employment were slightly over-represented, although these differences may be in part owing to the under-representation (in the original birth sampling frame) of ethnic minorities. Nathan (1999) summarised the results from analyses of the effects of attrition for the 1958 and 1970 birth cohort studies up to the 1991 (age 33) and 1996 (age 26) surveys, respectively. He concluded that, although cumulative attrition was substantial, the evidence only suggested substantial biases with respect to certain sub-groups.

Hawkes and Plewis (2006) analysed the timing of attrition and non-response. They found that children with more educated mothers were more likely to be lost early on in the study - the authors speculate that this may be because the children were at fee-paying schools and were consequently harder to trace. Most of the loss came early on, with the greatest drop coming between the ages of sixteen and twenty-three when there was a change in respondent from the parent to the cohort member themselves. After age 23, the likelihood of dropping out decreased and systematic observed differences those who stayed in and dropped out after this age were found to be small.

4.1.4 Advantages and disadvantages of the studies for the analysis of unequal pay

The three studies, taken together, offer important advantages in an analysis of trends in the unequal pay of women and men in Britain since the 1970s. A first major advantage is that they provide information on individual education, work and childbearing histories and family background characteristics, alongside information on adult earnings. Other surveys, such as the Family Expenditure Survey (FES) or the General Household Survey (GHS), do not contain the same level of detailed information on individual histories.

A second advantage is the comparability of the data collected in each of the studies, which allows meaningful comparisons to be drawn across the cohorts at similar ages. Again, pooled samples from the FES and/or GHS would not give as large a sample of individuals all of the same age (see table 4.14). Further, the timing of the cohorts allows the assessment of trends over a critical period, when equal pay and sex discrimination legislation was first introduced in Britain.

A third advantage is that the studies provide information on earnings for cohort members at several ages, covering a large part of the adult working lives of each cohort. Although the studies do not collect annual earnings data, the longitudinal aspect of the data offers some scope for analysing within-cohort trends and life-cycle wage dynamics. The British Household Panel Survey (BHPS) is a major source of longitudinal data on earnings, collecting these data annually, and has been used to investigate gender wage dynamics (Manning and Petrongolo, 2008; Brewer and Paull, 2006). The BHPS though only goes back to 1991, limiting the possibility for investigating life-cycle trends for older generations. It could also, again, only provide information either on much smaller sample sizes or much more loosely defined cohorts than the birth cohort studies. The New Earnings Panel Dataset (NESPD) does go back to 1975, and has been used to study women's wage dynamics (Connolly and Gregory, 2009), but does not contain the same level of detail on employment histories and family circumstances.

Although the birth cohort studies offer critical advantages for the present analysis, there are also drawbacks associated with using them.

- First, the datasets and metadata for the cohort studies are complex, and few derived variables are provided with the datasets. Consequently, the task of creating usable datasets is a large one.

- Second, as already outlined, considerable numbers of cohort members have left the studies permanently or have responded intermittently to surveys over time. The consequences are that samples have become less representative over time and that substantial amounts of data are missing for some cohort members. This may affect the external validity (generalisability) of the results and may also lead to biases.

- Third, the multi-purpose aspect of the birth cohort studies comes with disadvantages as well as advantages, compared to a survey focused mainly on employment or earnings. Across surveys (within cohorts), there have been changes in the wording of questions, the level of detail to which earnings information has been collected and the coding of the data. This is reviewed in the next section of the chapter.

- Fourth, the 1946 cohort data are particularly work-intensive to use. Historically, there has been less work done on documentation and much of the documentation was paper-based when this PhD work was undertaken, making the datasets a particular challenge to use. The employment history data in particular required a substantial amount of work to make usable. Currently, a major project is underway to fully document the datasets electronically and make them more widely available to the research community.

4.2 Data processing and description of variables

A substantial amount of work for this thesis went into sourcing the appropriate variables and creating usable and comparable datasets for earnings, employment experience and qualifications. This section describes the original variables used and the data processing undertaken. It also describes the final datasets and derived variables used in the analysis. Table 4.1 describes the surveys from which data have been used to construct earnings and employment histories. The sample sizes shown are the numbers who responded to the survey in question, not all of who responded to items on hours and earnings.

4.2.1 Current employment and full-time/part-time status

At each survey, cohort members were asked to describe their current main activity, either through a series of questions or through a single question accompanied by a list of different possible states. Tables 4.15-4.17 in the appendix to this chapter show the questions asked at each survey.

For each of the surveys, it is possible to derive two measures of part-time job status. One measure is based on self-defined full-time or part-time status. A second measure can be derived from reported basic weekly hours in a main job. The first measure has the advantage that part-time work is not defined solely with reference to basic working hours, but also takes into account the type of contract and the custom and practice of

Table 4.1: Description of surveys from which earnings and employment data were used

Birth year	Survey year(s)	Age	Survey method	N (men)	N (women)
1946	1972	26	interview	1,897	1,853
1946	1977 (1)	31	postal questionnaire	1,668	1,672
1946	1982	36	interview	1,650	1,657
1946	1989	43	interview	1,635	1,627
1958	1981/82 (2)	23	interview	6,268	6,271
1958	1991	33	interview	5,630	5,836
1958	1999/2000 (3)	41/42	interview	5,627	5,794
1970	1996 (4)	26	postal questionnaire	4,101	4,902
1970	1999/2000 (3)	29/30	interview	5,461	5,785
1970	2004/2005 (5)	34/35	interview	4,625	5,039

(1) Postal questionnaires were sent out in December 1977.
(2) Interviews were carried out between August 1981 and March 1982.
(3) Interviews were carried out between early November 1999 and March 2000.
(4) Questionnaires were sent out between April and July 1996.
(5) Interviews were carried out between February 2004 and June 2005.

the employer. For the 1958 and 1970 cohorts however, the self-report question was not entirely subjective. It included a prompt, with full-time work being defined as working 30 hours or more a week and part-time work being defined as working less than 30 hours a week. Using reported basic weekly hours, the standard definition of part-time hours in the UK is working less than 30 hours a week, excluding overtime. For teachers, the basic cut-off for full-time work is 25 hours in the classroom, excluding marking and preparation time.

Manning and Swaffield (2008) have found that the choice between the subjective and the objective measure did not make much practical difference in their analysis using Labour Force Survey data. Similarly, for the cohort data, figure 4.2 suggests a high level of agreement between the measures. The level of agreement is slightly lower for the 1946 cohort, to whom no definition of part-time work was given with the self-report questions. Half of those who defined themselves as part-time workers, but who were working full-time hours, were working exactly 30 hours a week. A similar level of disagreement in the opposite direction. Of those who defined themselves as full-time workers, but who were working less than 30 hours a week, nearly all were working 28 or 29 hours a week. There was a greater level of disagreement in measures for the small number of men defined as part-time workers on either measure than for women. The self-report measure was used for the analysis presented in this thesis.

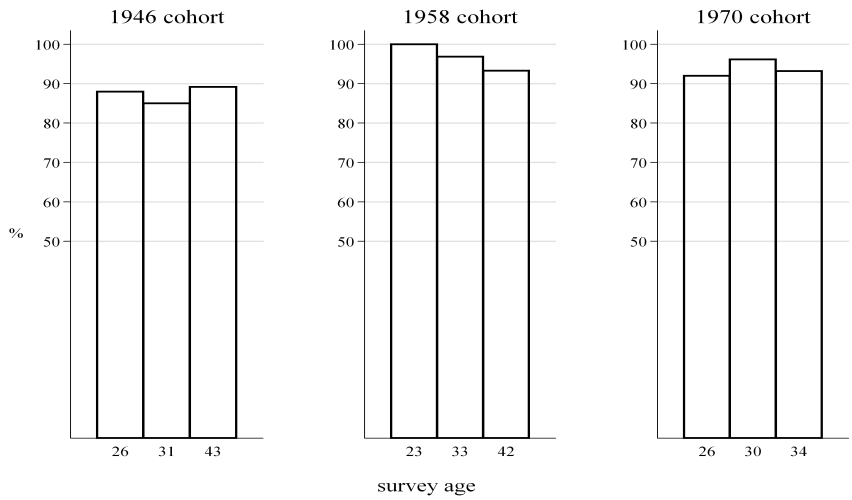

Figure 4.2: % of self-defined part-time employees working less than 30 hours a week (excluding overtime), by cohort and survey

4.2.2 Hourly earnings

Before-tax (gross) hourly earnings are used as the outcome measure in all of the analyses carried out in this thesis. The rationale is that before-tax earnings give a better indication of the financial value placed by employers on the work of their employees, and of the relative value placed on the work of women and men. After-tax (net) earnings are more sensitive to factors not related to the job, since they are directly affected by tax structures and policies that take into account household income, marital and parental status and hours of work. On the same grounds, income from self-employment is not included in the present analysis of earnings. In practice, employers are likely to respond to tax incentives when determining gross earnings, so these are also an imperfect measure of the rate at which the employer values an individual employee's work contribution, but arguably the least imperfect measure.

This section describes the original variables used and the algorithms applied to calculate gross hourly earnings. An overview of response rates to questions on earnings and hours of work is presented and signs of likely measurement error. A detailed description is given of the work done to impute continuous values for the 1946 cohort at age 43, using the banded earnings data from 1989 survey.

Questions on earnings and hours

At each of the adult surveys, cohort members have been asked for:

1. An estimate of their usual or most recent pay in their current job, using pay slips where available. With the exception of the 1996 (age 26) survey of the 1970 cohort, questions have been asked on pay before deduction of income tax and national insurance (gross) and on take-home (net) pay. Overtime pay is included in these figures.

2. Information on how long a period that usual or most recent reported pay covers i.e. a week, a calendar month, a year and so on.

3. Information about weekly hours of work; either a single question covering paid overtime or more than one question separately covering basic and overtime hours.

The wording of questions, the amount of information collected and the interviewer instructions have changed slightly across the surveys. Table 4.2 gives an overview of the information used. Earnings data collected at several other surveys were not used

.[4] Table 4.18 in the appendix to this chapter shows the wording of questions asked at each of the surveys.

Table 4.2: Information used from each survey to calculate gross hourly earnings

Cohort	Survey (age)	Information used
1946	1972 (26)	Typical gross earnings. Period covered by gross earnings. Basic rate in job. Typical weekly hours, exc. o/t. Regular & occasional o/t hours.
1946	1977 (31)	Average gross weekly earnings. Usual weekly hours, inc. o/t.
1946	1989 (43)	Banded estimate of average gross earnings (26 bands showing equivalent weekly, monthly and annual amounts). Average hours worked to earn reported pay. Average months worked in year. Hours worked in last week, inc. o/t.
1958	1981 (23)	Last gross pay. Usual gross pay. Pay period. Average weekly hours, inc. o/t.
1958	1991 (33)	Usual gross pay. Last gross pay. Corresponding pay periods. Usual weekly hours, inc. o/t.
1958	2000 (42)	Last gross pay. Pay period. Usual weekly hours, inc. o/t.
1970	1996 (26)	Usual net pay. Pay period. Usual weekly hours, inc. o/t.
1970	2000 (30)	Last gross pay. Pay period. Usual weekly hours, inc. o/t.
1970	2004 (34)	Last gross pay. Pay period. Usual weekly hours, inc. o/t.

Calculation of hourly earnings

A measure of gross hourly earnings has been calculated in two stages.

1. Gross (before-tax) reported earnings are divided by the number of weeks contained in the corresponding reported pay period to get a figure for gross weekly earnings. e.g. For someone with an annual salary of £34,000, the corresponding weekly estimate is £34,000 a year/52 weeks = £653.85 a week

2. Gross weekly earnings are divided by weekly hours, including paid overtime, but excluding unpaid overtime, to obtain an estimate of gross hourly earnings. e.g. For the same individual earning £653.85 a week and working a 38 hours week (including overtime), the estimate of hourly earnings is £653.85 a week/38 hours = £17.21 an hour.

[4]Earnings data were collected at ages 36 and 53 from the 1946 cohort but in banded net form for the former. Data on hours collected via a telephone survey of the 1958 cohort in 2004 (age 46) had unacceptably high numbers of missing items owing to a problem with the telephone interview software.

Self-employed individuals are counted as employed for purposes of comparing employment rates, but their earnings are treated as missing. For some of the analyses, potential wages from employment (vs. self-employment) have been estimated for self-employees.

Each measure of hourly earnings was adjusted to January 2000 prices using the long-term indicator of prices of goods and services produced by Office for National Statistics.[5] By adjusting earnings for price inflation, the wage trends that are analysed represent real aggregate earnings growth (i.e. average growth in the earnings of employees relative to average consumer prices), as well as changes in relative earnings by age, cohort and gender.

Item non-response and measurement error for earnings and hours

Table 4.3 shows high response rates to questions on gross earnings and hours of work amongst employees (excluding self-employed) at each survey. The response rates to questions on earnings and hours were slightly lower for the 1977 postal survey of the 1946 cohorts. Response is understated in these figures, since a substantial fraction of individuals who did not report before-tax (gross) earnings did report after-tax (net) earnings.

Table 4.3: Response rates to questions on before-tax (gross) earnings and hours

Birth year	Survey (age)	Earnings questions	Hours question	Both	N (1)
1946	1972 (26)	92.9 %	93.3 %	90.0 %	2,448
1946	1977 (31)	87.4 %	90.8 %	84.5 %	2,147
1946	1989 (43)	98.0 %	99.1 %	97.1 %	2,299
1958	1981 (23)	95.3 %	97.3 %	93.0 %	8,616
1958	1991 (33)	91.2 %	96.6 %	89.9 %	7,676
1958	2000 (42)	91.8 %	95.8 %	88.3 %	8,216
1970	1996 (26)	90.9 %	98.9 %	90.2 %	6,675
1970	2000 (30)	92.8 %	96.8 %	90.1 %	8,264
1970	2004 (34)	90.0 %	99.2 %	89.4 %	7,052

(1) This figure is the number of cohort members who responded to the survey in question and were employed (excluding self-employed) at the time of the survey.

[5] Available at http://www.statistics.gov.uk/StatBase/TSDdownload1.asp.

Likely reporting and coding error is inferred from gross wage values which are implausibly high or low and/or which are strongly out of line with reported net earnings. Table 4.4 gives information on the percentage of cases with very high (more than £500 an hour) and very low (less than 50p an hour) gross wage values in each survey and those that are high (> £100) or low (<£2). Table 4.5 shows the percentage with implausible gross-to-net hourly wage ratios (ratio is <0.4 or >4). These suggest a small amount of measurement error. Its likely effects on wage estimates are assessed in the final part of this chapter.

Table 4.4: Percentage with very high or low estimated hourly earnings (1)

Birth year	Survey (age)	>£500	>£100	<£2	<50p	N (2)
1946	1972 (26)	0.0 %	0.0 %	1.4 %	0.0 %	2,202
1946	1977 (31)	0.0 %	0.0 %	1.0 %	0.0 %	1,814
1946	1989 (43)	0.0 %	0.1 %	2.1 %	0.1 %	2,233
1958	1981 (23)	0.0 %	0.0 %	0.8 %	0.0 %	8,012
1958	1991 (33)	0.1 %	0.8 %	0.6 %	0.1 %	6,901
1958	2000 (42)	0.2 %	0.9 %	2.2 %	0.5 %	7,241
1970	1996 (26)	0.0 %	0.1 %	0.7 %	0.1 %	6,020
1970	2000 (30)	0.1 %	0.6 %	2.4 %	0.5 %	7,439
1970	2004 (34)	0.1 %	0.7 %	1.0 %	0.1 %	6,307

(1) The percentages are calculated after adjusting to 2000 prices.
(2) This figure is the number of employees with an observed hourly wage.

Table 4.5: Percentage with odd gross-to-net hourly wage ratio, by survey (1)

Birth year	Survey (age)	Odd ratio (1)	N (2)
1946	1972 (26)	0.0 %	2,137
1958	1981 (23)	0.1%	7,911
1958	1991 (33)	1.8%	6,825
1958	2000 (42)	3.6%	7,223
1970	2000 (30)	3.5%	7,422
1970	2004 (34)	1.5%	6,292

(1) An odd ratio is defined as one of ratio>4 or <0.4, in line with work by Alissa Goodman (see Bynner et al., 2001). Ratios can only be calculated for surveys where information on both gross and net earnings was collected.
(2) This figure is the number of employees with an observed gross and net hourly wage.

To investigate the effects of potential measurement error on wage estimates, the earnings data for the 1958 and 1970 cohorts collected in the 2000 survey were altered, using a series of edits devised by Lorraine Dearden and Alissa Goodman at the Institute for Fiscal Studies (see Bynner et al., 2001). Implausibly high or low values for recorded gross earnings and those strongly out of line with reported net earnings were replaced with more plausible values according to the suspected coding error. For example, where the pay period reported for gross pay differed from the pay period reported for net pay, and gross pay was implausibly high or low, the gross pay period was replaced with the net pay period. However, statistical analyses based on these edited data were found to give qualitatively similar results to those using the unedited datasets, once extreme values were excluded. The analysis presented in the thesis are based on the unedited earnings datasets, including only additional imputed values for cases where net earnings were observed and gross earnings were missing (see below).

Imputation of incomplete wage data for the 1946 and 1970 cohort

A substantial amount of work was done to create a set of complete earnings data for the 1946 cohort at age 43. As part of the 1989 survey, cohort members were asked a single question about their earnings. They were presented with a card showing 25 intervals - 23 with specified upper and lower bounds and two open-ended intervals at the bottom and top of the distribution. Intervals were divided into £1000 bands for annual earnings, with equivalent monthly and weekly sums. The bottom and top bands were open-ended, capturing earnings of less than £2,000 a year (£40 a week) and more than £25,000 a year (£481 a week) respectively.

Table 4.6 shows the distribution of gross annual earnings across the 25 bands for employees in the 1989 survey of the 1946 cohort (weighted and unweighted) and for sub-samples of employees aged 40-45 in the 1989 Family Expenditure Survey (FES) and General Household Survey (GHS). In 1989, 18.5 per cent of men in the cohort sample had earnings which fell into the top open-ended interval, representing 15 per cent of the population once the sample is weighted to adjust for stratification. This relatively high fraction in the top band, compared to distributions for the whole population, reflects the combined effects of stratification, i.e. the oversampling of men from higher social class backgrounds, and, more importantly, the higher earnings of older male employees. Around 13 per cent of women's earnings fell in the bottom open-ended interval. A quarter of female part-time employees (half of female employees) had earnings in this lowest band.

Table 4.6: Cumulative percent in each earnings band, 1946 cohort, FES and GHS, by sex

	Women				Men			
Earnings band	1946 cohort		FES	GHS	1946 cohort		FES	GHS
(£per annum)	(1)	(2)	(3)	(3)	(1)	(2)	(3)	(3)
less than 1,999	12.6	13.4	16.5	13.8	0.4	0.4	0.2	0.0
2,000-2,999	25.3	26.8	27.2	26.6	0.9	0.7	1.0	0.7
3,000-3,999	35.6	36.9	37.9	36.8	1.1	0.9	1.2	1.1
4,000-4,999	44.8	46.1	46.0	44.9	1.4	1.3	1.4	1.7
5,000-5,999	53.0	54.7	52.5	53.9	2.9	2.7	2.7	3.2
6,000-6,999	60.6	62.0	61.6	61.7	5.2	5.2	4.7	6.5
7,000-7,999	66.4	68.9	66.4	68.3	7.7	7.5	6.1	9.2
8,000-8,999	72.6	74.8	69.3	75.3	11.4	11.3	11.3	13.5
9,000-9,999	76.8	78.8	72.7	79.7	16.0	16.8	17.2	19.9
10,000-10,999	80.2	82.5	75.4	82.0	23.2	25.3	24.5	28.9
11,000-11,999	82.8	85.1	78.1	84.5	28.6	31.4	30.2	35.4
12,000-12,999	85.8	87.8	80.4	87.8	35.2	38.9	35.1	43.5
13,000-13,999	88.7	90.1	82.3	90.0	41.0	45.0	40.2	49.3
14,000-14,999	91.6	92.6	84.2	92.1	47.6	51.4	45.3	53.7
15,000-15,999	94.5	95.2	86.3	94.8	53.5	57.7	49.7	58.9
16,000-16,999	95.9	96.5	87.4	96.2	57.7	61.9	54.6	64.1
17,000-17,999	96.8	97.2	88.2	96.9	61.8	66.5	60.3	68.5
18,000-18,999	97.3	97.8	88.7	97.4	66.8	71.3	64.4	74.1
19,000-19,999	98.1	98.7	89.9	97.7	69.2	73.3	68.4	76.6
20,000-20,999	98.4	98.9	90.1	98.0	72.9	77.2	71.6	80.3
21,000-21,999	99.0	99.2	90.4	98.5	74.9	79.5	73.8	83.4
22,000-22,999	99.0	99.2	90.6	100.0	77.1	81.0	75.3	85.5
23,000-23,999	99.1	99.4	90.9	100.0	79.4	83.3	77.7	86.3
24,000-24,999	99.1	99.4	100.0	100.0	81.6	85.1	80.0	88.7
25,000+	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0
Sample size	1,157	-	593	737	1,121	-	592	710

The horizontal lines mark off the 25th, 50th and 75th percentiles of the female and male earnings distributions. (1) Percentages are unweighted. (2) Percentages are weighted. (3) Sub-samples of individuals aged 40-45 from the 1989 FES and 1989-90 GHS.

The banding of the earnings data results in a loss of information. Micklewright and Schnepf (2007) quantified the loss of information associated with banding of single-question income data, relative to collection of more detailed information via multiple questions. They used detailed data on individual incomes from the Family Resources Survey (FRS) and the Expenditure and Food Survey (EFS) to analyse the effects of banding the data. Using methods to decompose income variation into within-band and between-band components, they found some loss of information, with a more substantial loss of information at the top of the distribution, coming from the top unbounded interval (containing around 8% sample). The implications for the present analysis are two-fold: first, the greater concentration of the male earnings in the top, unbounded interval is likely to lead to a greater loss of information; and second, measures of the gender pay gap that weight the top of the distribution more heavily (i.e. differences in means vs. medians) are likely to be more sensitive to this loss of information.

It has become fairly standard to use interval regression methods to model banded income data as a dependent variable (e.g. Dearden et al., 2003; Micklewright and Schnepf, 2007). Interval regression simultaneously estimates within-band values of earnings and the model parameters by maximum likelihood, using the assumption that earnings are log-normally distributed.[6] However, testing the interval regression model on grouped data for the FES, GHS and the 2000 survey of the 1958 cohort (at age 42) suggested that, for older, male earnings, the results of interval regression are sensitive to the fraction of cases observed in the top open-ended interval. The greater this fraction, the lower the estimated intercept and the higher the estimated coefficients on covariates. This may be because model-based approach requires strong distribtional assumptions, i.e. log-normality, and that small departures from this become more important when a larger fraction of the data are censored. After experimenting with a number of approaches, I instead imputed within-band values for cohort members using unbanded data from the FES and GHS, rather than modelling the intervalised data. Imputation using unbanded earnings data is more likely to restore the underlying shape of the earnings distribution.

The approach taken was to replace observed banded income for cohort members with imputed continuous values using the 1989 Family Earnings Survey (FES) and the 1989 General Household Survey (GHS). The FES and GHS samples were restricted to employees aged between 40 and 45. Propensity-score matching was used to identify individuals who were most similar within each band, using information on gender, weekly hours of work, region of residence and occupational status. Samples of between 100 and 200 individuals were used in 25 separate probit models to estimate propensity scores and match on these scores, separately for each band. With these small sample sizes, only weekly hours of work and gender appeared as significant determinants of within-band variation in earnings. Consequently, the within-band values can be viewed in many cases as random draws from the within-band distributions. This is likely to introduce more uncertainty into estimates based on these imputed continuous data, but it is not obvious that it generates any particular biases.

[6]The Stata program *intreg* can be used to fit these models. This is based on an two-step estimator proposed by Stewart (1983).

For the 1970 cohort, the postal survey sent out in 1996 at age 26 asked cohort members about their net earnings, but not their gross earnings. Again, missing values for gross earnings were replaced with observed values taken from external datasets. Gross earnings data were used from the Labour Force Survey (LFS) for 1996 (four quarters) and the first quarter of 1997. The LFS sample was restricted to employees aged between 20 and 40. First, the samples were divided into eight groups, classified by gender, marital and parental status, on the basis that these characteristics, as well as the level of earnings, affect the tax rate (and therefore the ratio of gross to net earnings). Second, each cohort member was matched to their nearest neigbour in the LFS (in the same group) based on their observed net weekly wage. Third, missing gross weekly earnings for each cohort member were replaced with the value of gross earnings observed for their match in the LFS.

Table 4.7 shows mean net and gross earnings for the cohort sample and the matched LFS sample. The large LFS samples meant that near exact matches on net weekly earnings were made in each case. The average ratio of gross-to-net earnings is around 1.4 and did not vary significantly across the groups. For the final cohort sample of employees, the ratio of women's to men's median weekly earnings was 0.78 based on imputed gross earnings, compared to 0.8 based on net earnings. However, the difference in estimated ratios was not statistically significant.

Table 4.7: Mean net and gross weekly earnings by gender, marital and parental status, matched samples from the 1996 survey of the 1970 cohort and the 1996 LFS

	Sample sizes		Net earnings (£)		Gross earnings (£)	Gross-to net ratio
Group	1970 cohort	LFS	1970 cohort	LFS	LFS	LFS
Women						
Not married, with children	235	1,339	132.8	133.1	172.1	1.3
Married, with children	342	4,112	125.3	125.4	172.2	1.4
Not married, no children	1,952	3,089	208.0	212.7	312.0	1.5
Married, no children	730	1,561	199.4	200.6	291.2	1.4
Men						
Not married, with children	154	672	234.6	234.4	412.1	1.8
Married, with children	308	4,055	253.2	252.9	358.3	1.4
Not married, no children	1,937	3,894	248.5	251.4	357.0	1.4
Married, no children	409	1,328	264.0	262.7	362.4	1.4

Values are in 1996 prices.

For the 2000 surveys of the 1958 and 1970 cohorts, values for gross hourly earnings have also been imputed for cases with missing values using observed values for net earnings. This was based on code written by Lorraine Dearden and Alissa Goodman (see Bynner et al. (2001) for details).

4.2.3 Employment experience

Estimates of the number of years spent in paid employment and the number of years in full-time and part-time work have been used throughout the analyses in this thesis. To estimate these figures, I created employment histories for the 1958 and 1970 cohorts using retrospective job and unemployment history data collected from cohort members at two or three surveys in adult life.[7] For the 1946 cohort, the job history data were too incomplete for this approach, particularly the data collected via postal surveys when cohort members were in their late teens and early twenties. Instead, I estimated lengths of time spent in paid employment from the age of twenty-five.

1946 cohort employment histories

The work experience variables constructed for members of the 1946 cohort include estimates of the length of time spent in employment from the age of twenty-five. An attempt was made to construct complete employment histories since leaving school, but the data were too incomplete for this approach. Around two thirds of the cohort left school and entered the labour market between the ages of 15 and 18. In their teens and early twenties, cohort members were asked to update employment histories in a series of frequent postal questionnaires. A further complication was that different questionnaires were sent to those who were in full-time education and those who had left school by their teens. As a consequence, data collected between the ages of 16-25 are incomplete, not fully coded and not all available electronically. A derived variable is available in the datasets containing information on the number of months out of the labour market between the ages of 18 and 25. In the final part of this chapter, this variable is used in an assessment of the likely effects of the truncation of the employment experience variables at age twenty-five on the various analyses of unequal pay.

The task of constructing the employment histories was carried out in several steps. Table 4.8 gives an overview of the original survey data used to construct the employment histories. Table 4.19 in the appendix to this chapter shows the actual questions that

[7]The job histories created by Kelly Ward and available from the UK Data Archive were not suitable for my purposes since these focus on job and occupation data and do not make full use of the unemployment histories.

were asked at each survey. First, the original variables containing the information shown in table 4.8 were collected into a single data file, including start dates for current jobs, the length of time spent out of work and retrospective job and unemployment histories collected via the 1982 and 1989 surveys.

Table 4.8: Summary of information used to construct employment histories for the 1946 cohort

Survey (age)	Information used	Sample size (1)
1972 (26)	Start date of current job. FT/PT status. Length of current spell out of work. Number of jobs held 1971-72. Number and length of spells out of work 1971-72.	3,750 (3,729)
1977 (31)	Start date of current job. FT/PT status. Number of jobs held 1972-77.	3,340 (3,205)
1982 (36)	Start date of current job. FT/PT status. Start and end dates and FT/PT status for up to 7 previous jobs since last job on record (from 1977 or 1972). Start and end dates for up to 4 non-employment spells since last recorded spell.	3,322 (3,307)
1989 (43)	Start date of current job. FT/PT status. Start and end dates and FT/PT status of job held in 1982 (if not interviewed then) and longest job held between 1982 and 1989. Start and end dates for up to 6 non-employment spells since last recorded spell.	3,262 (3,248)

(1) This figure is the number who responded to the survey. The figure in brackets is the number who responded to questions on employment.

Second, the start and end dates for job and non-employment spells collected via the 1982 and 1989 surveys were converted into century month code (CMC) [8] and organised into chronological order. The job and non-employment spells were combined using an iterative algorithm (looping over consecutive values in Stata) to form a single work history. Partially complete or conflicting start or end dates were imputed or amended using the following principles:

1. Use June as the start or end month where the start or end year for a spell is given, but the start or end month is missing. This was for the purposes of converting all dates into century month code (CMC).

2. Use the start date of a next spell for a missing end date of a current spell.

[8] DATE [CMC] = ((YEAR-1900) x 12) + MONTH. Thus, January 1900 is coded '1', February 1900 is coded '2' and March 1946 is coded '555', and so on.

3. Allow non-employment spells to be 'nested' in reported job spells.
4. Where the end date for one spell is later than the start date for a subsequent (different) spell, assume start date to be correct.

Third, work experience was estimated separately for four work-history segments covering the periods: March 1971- March 1972; April 1972- March 1977; April 1977- March 1982; and April 1982- March 1989. In cases where the start date for a current job pre-dated the start date for the work segment in question, the whole of the period could simply be counted as time spent in work. In other cases, a mixture of information was used to estimate time spent in and out of work, including: current employment status (reported at the current survey); spells in and out of work from the employment history (reported at the current or the subsequent survey); and, for the first segment only, the length of time out of work in the past year and the number of jobs held. For the first segment, full-time and part-time experience was estimated based on current job status. For other work segments, full-time and part-time experience was either calculated directly from the reported job histories or was estimated based on the fraction of reported time spent in full-time or part-time work for that work segment i.e. if FT/PT status were missing for one of the jobs, the fraction of time spent in FT and PT work for the other jobs in the relevant work segment would be applied to the whole period. Finally, the four work-segments were combined, as appropriate, to generate work experience variables for ages twenty-six (in 1972), thirty-one (in 1977) and forty-three (in 1989).

1958 and 1970 cohort employment histories

For the 1958 cohort, retrospective employment history data were collected at ages 23, 33 and 42. I used the retrospective employment history data collected at ages 33 and 42 to estimate work experience totals for these ages. I used the derived variables provided with the dataset at age 23 (see Centre for Longitudinal Studies (1981)).

In 1981, at age 23, cohort members were asked for the start and end dates for up to four jobs and for up to four periods of unemployment. The questions are shown in table 4.20. In 1991, at age 33, cohort members were asked to complete a questionnaire entitled 'Your Life Since 1974'. This questionnaire asked cohort members to fill in start and end dates for up to twelve paid jobs and, separately, for twelve periods out of paid employment since the age of 16 (in 1974). In 2000, at age 42, cohort members were asked in the main face-to-face interview about start dates of up to ten previous spells engaged in different economic activities (jobs and non-employment), dating back to the previous interview at age 33 (in 1991).

I combined the job and unemployment histories to create a single employment history. The set of principles applied to the 1946 employment histories was also used for the 1958 and 1970 employment histories to impute or amend incomplete or conflicting start and end dates. Where the start dates for spells reported at age 42 conflicted with those reported at age 33, the age 33 data were treated as more likely to be correct, being closer to the period in question.

For the 1970 cohort, retrospective employment history data were collected at ages 30 and 34. In the 2000 interview at age 30, the question was the same as for the 1958 cohort survey in that year, requesting start dates of up to ten previous spells of employment or non-employment (including full-time education) from the age of 16 onwards. In the 2004 interview at age 34, cohort members were asked to update economic activity histories or to recall full histories if they had not been interviewed in 2000. Information on up to ten spells was collected. I again applied the same set of principles to create a single employment history.

4.2.4 Highest educational qualification

Measures of the highest qualification achieved at each age, including academic and vocational qualifications obtained in adult life, are used in both of the main analyses in this thesis. Some work was required to group the large number of different types and levels of qualifications into broad, comparable categories. This exercise was complicated by changes in the education system, changes in the content of particular qualifications and changes in the recognised classification of qualifications over the period covered. The majority (more than 60%) of the 1946 cohort left school at age 15 or 16, of whom a minority had O-levels. Around 60% of the 1958 cohort left school at 16 (the minimum school-leaving age for this cohort), the majority with O-levels. The 1970 cohort were the last cohort to take O-levels and CSEs - GCSEs were introduced in England and Wales in September 1986. Around a fifth of the cohort left school without qualifications.

Cohort members have been asked in detail about types and grades of academic and vocational qualifications gained throughout their teens and adult life. In the 1972 interview (age 26), members of the 1946 cohort were asked in detail about qualifications they had obtained since leaving school. In surveys conducted at ages 36 and 43, they were asked a shorter question asking about any qualifications gained since they had last been interviewed. The 1958 cohort members were asked in detail about their qualifications in all adult surveys. The 1991 survey (age 33) additionally collected information about all qualifications obtained from age 16 onwards, as well as qualifications obtained since the 1981 survey (age 23). The 2000 survey (age 42) asked cohort members about qualifications obtained since the 1991 survey (or since the age of 16 for those who had not been interviewed in 1991). The 1970 cohort members were asked a short question about their qualifications in the postal survey sent out in 1996 (age 26). In the 2000 survey (age 30), they were asked in detail about any qualifications gained since the age of 16. In the 2004 survey (age 34), they were asked about any qualifications gained since they were last interviewed and about their highest qualification.

I have used adapted versions of two general classification frameworks, which have previously been used to construct two alternative measures of highest qualification for the cohort studies. The first corresponds broadly to National Vocational Qualifications (NVQ) levels, which are designed to reflect different levels of competence, covering both academic and vocational qualifications. The second is based on the classification of qualifications used in the General Household Survey (GHS). This differs from the NVQ framework in distinguishing between degree-level qualifications, on the one hand, and diplomas in higher education and equivalents, including some nursing and teaching qualifications. The latter are classified as below degree-level. The GHS classification also groups NVQ level 1 qualifications together with no formal qualifications. Table 4.9 shows the classification used for the analysis in this thesis, based on Makepeace et al. (2003).

For the 1946 cohort, a variable containing the highest academic or vocational qualification achieved by 26 years uses the Burnham scale (Department of Education and Science, 1972). This contains eight levels: no qualifications, below ordinary secondary qualifications, ordinary secondary qualifications (O-levels and their vocational equivalents), advanced secondary education (A-levels and their equivalents), professional qualifications (below degree-level), degree-level qualfications, masters and doctorates. I have used a mapping of these eight categories onto the NVQ and GHS frameworks, devised by Gerald Makepeace and Andrew Jenkins (Makepeace et al., 2003). I updated the highest qualification measure for the 1946 cohort at age 43 using information collected in the 1982 (age 36) and 1989 (age 43) surveys. Information about qualifications was not collected via the postal survey in 1977 (age 31), so the variable used at this age is the highest qualification achieved by age 26.

Some work was required to modify existing derived measures of highest qualification

Table 4.9: Classification of qualifications

Group	Academic	Vocational	1946 cohort
Degree or above	Higher Degree, Degree, PGCE	NVQ level 5	Burnham A1, Masters, Doctorate, First Degree, Dip Tech etc.
Diploma or equivalent	HE diploma, Teacher training qualification (not PGCE)	BTEC Higher Certificate/Diploma, HNC/HND NVQ level 4, Professional degree-level qualifications, Nursing/paramedic, RSA Higher Diploma	Burnham A2, Professional (surveying etc)
A-level or equivalent	1 or more A level, 2 or more AS levels, Scottish Highers, Scottish Certificate of 6th Year Studies	BTEC National Diploma, ONC/OND, City & Guilds Part 4/Career Ext/Full Tech, City & Guilds Part 3/Final/Advanced Craft, City & Guilds Part 2/Craft/Intermediate, RSA Advanced Diploma, Pitmans level 3, Advanced GNVQ, NVQ level 3	A level, Burnham B
O-level or equivalent	GCSE grade A*-C, O levels grade A-C, CSE grade 1, Scottish Standard grades 1-3, Scottish lower or ordinary grades	BTEC First Certificate, BTEC First Diploma, City & Guilds Part 1, Apprenticeships, RSA First Diploma, Pitmans level 2, Intermediate GNVQ, NVQ level 2	1 or more O-levels or training equivalent, Burnham C
Below O-level	O levels grade D-E (not GCSE), GCSE grade D-G, CSEs grades 2-5, Scottish Standard grades 4-5, Other Scottish school qualification	City & Guilds/Other, RSA Cert/Other, Pitmans level 1, Other vocational qualifications, HGV Foundation GNVQ/Other GNVQ, NVQ level 1/Other NVQ, Units towards NVQ	Nominal, Sub O-level, Below Burnham C

included in the 1958 and 1970 datasets, since the framework for classifying qualifications has changed over the period covered. For example, highest qualification measures available in the datasets for the 1981 and 1991 surveys of the 1958 cohort classify a first bachelors degree as NVQ level 5 (see Smith, 1991, part 5, p.31). In contrast, measures of highest qualification created for the 2000 surveys of the 1958 and 1970 cohorts classify a first bachelors degree as NVQ level 4 (Bynner et al., 2001, p.44). This is because the NVQ framework changed between 1991 and 2000. Makepeace et al. (2003) also point out that the form of the survey questions asked about qualifications has also changed alongside changes in the structure of qualifications. I have used notes and code produced by Andrew Jenkins, Gerald Makepeace and Samantha Parsons to group the different qualifications up to 2000 for the 1958 and 1970 cohorts, using the more recent NVQ framework (Bynner et al., 2001, pp.39-62). For the 2004 survey of the 1970 cohort, Augustin de Coulon generously shared his code to group qualifications by NVQ level. I adapted this to group the qualifications using the classification shown in table 4.9.

4.2.5 Description of the final datasets

Table 4.10 gives the definitions of variables used in the analyses. In addition to data on wages, employment experience and highest qualification, additional information has been used on job tenure, parental status, social class of first job, maths and reading ability at age 10 or 11 and childhood family circumstances. Most of this information has been coded into variables in the original datasets that can be used, with minimal adaptation, in the present analysis. Where items are missing for categorical variables, an extra category has been used. Variables containing information on the number and ages of children in the household were derived from information on parental histories and current household structure. Scores from mathematics and reading tests taken at age ten or eleven are used as indicators of educational achievement at these ages. Similar to Joshi et al. (2007), I have standardised the scores to make them comparable, with a mean zero and standard deviation 1 derived from the sample of cohort members (girls and boys) who took the test. Observations with missing values were given a standardised score of zero and a dummy variable was included in models, indicating that the information was missing.

Table 4.10: Variable definitions

Gross hourly wage	Derived gross wage per hour worked (including overtime) (£)
Part-time worker	Dummy = 1 if self-defined part-time worker
Work experience	Years in paid employment up to time of survey since age 16 for the 1958 and 1970 cohorts and since age 25 for the 1946 cohort
Full-time experience	Years in full-time paid employment since age 16 (1958 and 1970 cohorts) or 25 (1946 cohort). Full-time work is self-defined
Part-time experience	Years in part-time paid employment since age 16 (1958 and 1970 cohorts) or 25 (1946 cohort). Part-time work is self-defined
Job tenure	Years working for current employer at time of survey
O-level or equivalent	Dummy = 1 if highest qualification = O-level or equivalent at time of survey
A-level or equivalent	Dummy = 1 if highest qualification = A-level or equivalent at time of survey
Diploma	Dummy = 1 if highest qualification = diploma from non-degree higher education at time of survey
Degree or higher	Dummy = 1 if highest qualification = Bachelors degree, equivalent or higher at time of survey
Maths score at age 11	Standardardised score (z-score) from maths test taken at age 10 (1970 cohort) or 11 (1946 and 1958 cohorts)
Reading score at age 11	Standardardised score (z-score) from reading test taken at age 10 or 11
Missing maths score	Dummy = 1 if maths test not taken or score from test missing
London or SE	Dummy = 1 if living in London or the South East at time of survey
Children in hhld	Dummy = 1 if own or other children living in household at time of survey
Children under five	Dummy = 1 if children under five years living in household at time of survey
More than one child	Dummy = 1 if more than one child living in household at time of survey
Cohabiting	Dummy = 1 if living with spouse or partner at time of survey
Social class of first job	
I	Dummy = 1 if first job in RG Class I
II	Dummy = 1 if first job in RG Class II
III	Dummy = 1 if first job in RG Class II
IV	Dummy = 1 if first job in RG Class IV
V	Reference category
VI	Dummy = 1 if first job in RG Class VI
Missing	Dummy = 1 if information on occupation of first job missing

Table 4.10: Variable definitions - continued

Father in non-manual job	Dummy = 1 if father in non-manual occupation at birth for 1946 cohort
Father's social class:	
I	Dummy = 1 if father in RG Class I
II	Dummy = 1 if father in RG Class II
III	Dummy = 1 if father in in RG Class II
IV	Dummy = 1 if father in RG Class IV
V	Reference category
Mother's age at birth:	
Youngest quartile	Reference category
Second quartile	Dummy = 1 if mother's age in second quartile of age distribution
Third quartile	Dummy = 1 if mother's age in third quartile of age distribution
Oldest quartile	Dummy = 1 if mother's age in top quartile of age distribution
Missing	Dummy = 1 if information on mother's age at birth missing
Mother's education:	
Left before 16	Reference category
Left at 17	Dummy = 1 if mother left school at age 17
Left at 18	Dummy = 1 if mother left school at age 18 or older
Missing	Dummy = 1 if information on mother's schooling missing
Father's education:	
Left before 16	Reference category
Left at 17	Dummy = 1 if father left school at age 17
Left at 18	Dummy = 1 if father left school at age 18 or older
Missing	Dummy = 1 if information on father's schooling missing
Number of siblings at age 16	
Only child	Dummy = 1 if no siblings at age 16
One sibling	Dummy = 1 if one sibling at age 16
Two or three siblings	Dummy = 1 if two or three siblings at age 16
Four or more siblings	Reference category
Older siblings at age 16	
No older siblings	Dummy = 1 if no older siblings at age 16
One older sibling	Dummy = 1 if one older sibling at age 16
Two or more	Reference category

The figures and analyses presented in this thesis use the following samples:

1. The core cross-section samples include all cohort members who responded to the relevant survey, shown in table 4.1. Simple descriptions of proportions in work and proportions of women with children are based on these samples.

2. The cross-section samples used to estimate the propensity-score models in chapter 5 exclude individuals with missing values for highest qualification or employment experience. Women with missing values for items on the presence of children in the household are also excluded.

3. The cross-section samples used to estimate the wage models in chapter 7 exclude individuals who are not employed or who are self-employed. They also exclude those who have missing values for hourly wages, highest qualification or employment experience.

4. The longitudinal samples used to estimate wage trends for the 1958 and 1970 cohorts in chapter 7 include individuals who took part in all three of the adult surveys and who were employees with observed wages at the time of each survey.

Table 4.11 shows the sample sizes for each of these four samples. The next section assesses the representativeness of each of these samples using information on maths and reading scores in childhood and wages in adulthood.

Table 4.11: Sample sizes used in different analyses, by gender

Birth cohort	Survey (age)	(1)		(2)		(3)		(4)	
		Men	Women	Men	Women	Men	Women	Men	Women
1946	1972 (26)	1,897	1,853	1,768	1,710	1,385	692	-	-
1946	1977 (31)	1,668	1,672	1,467	1,431	974	514	-	-
1946	1989 (43)	1,635	1,627	1,283	1,167	902	836	-	-
1958	1981 (23)	6,268	6,271	5,535	5,721	4,263	3,514	1,965	1,392
1958	1991 (33)	5,630	5,836	5,441	5,682	3,672	3,128	1,965	1,392
1958	2000 (42)	5,627	5,794	5,585	5,764	3,957	3,998	1,965	1,392
1970	1996 (26)	4,101	4,902	3,367	3,836	2,375	2,841	1,505	1,451
1970	2000 (30)	5,461	5,785	5,360	5,735	4,166	3,894	1,505	1,451
1970	2004 (34)	4,625	5,039	4,588	5,014	3,289	3,004	1,505	1,451

4.3 Assessment of data quality

This final section asssess the quality of the datasets, including:

- the effects of the stratification of the 1946 sample on wage estimates;
- the potential effects of measuring work experience since age 25 rather than since leaving full-time education on estimates of unequal treatment of women and men for the 1946 cohort ; and
- the combined effects of attrition, survey non-response and missing data on the representativeness of the samples and on wage estimates based on these.

The treatment of these data quality issues is described in each case.

4.3.1 Stratification of the 1946 cohort sample

A smaller, stratified sample of the 1946 cohort was followed-up in 1948 (age two) and has formed the core target sample that has been tracked ever since. The sample included 5,632 children (2,547 girls and 2,815 boys) out of the original 16,695 births in the target week. All children born to fathers in non-manual and agricultural occupations were included and one in four of children born to fathers in urban, manual occupations, the aim being to preserve roughly equal numbers from the two social class groups (Wadsworth, 1991). Three-quarters of the fathers of the whole birth cohort were at this time in manual occupations. Babies born to unmarried mothers (672), most of whom had been adopted in this cohort, and multiple births (180) were excluded from the study.

Class origins, the basis upon which the sample was stratified, are strongly associated with future earnings. Further, their effects differ by gender. Figure 4.3 shows mean hourly earnings by father's occupational class at birth, separately at each survey for women and men. There are clear differences by social class origin, which differ by gender and appear to increase with age for men. As well as these straightforward differences, social class origins are also likely to mediate the effects of other characteristics, such as educational attainment, on wages. Consequently, estimates of unequal treatment of women and men may also differ by social class origin.

A weighting variable (INF) is provided with the 1946 cohort datasets. This variable contains information on the frequency with which each individual in the stratified

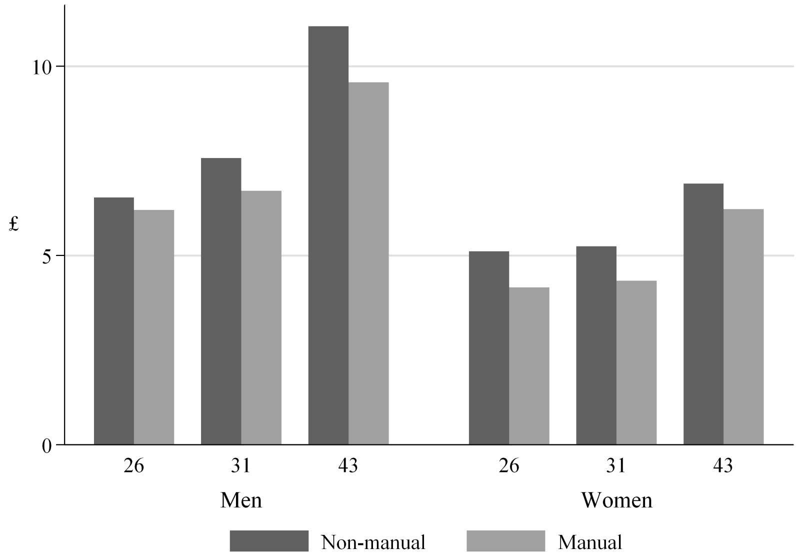

Figure 4.3: Mean hourly earnings by age, gender and father's occupational class at birth, 1946 cohort (£, 2000 prices)

sample appears in the target population (i.e. the whole cohort). It takes the value 4 for cohort members whose fathers were in urban, manual occupations at birth, i.e. those who were undersampled, and 1 for those whose fathers were in non-manual or agricultural occupations. This variable has been used in the different analyses in the following ways:

- To be representative of the class origins of the whole cohort (not just those included in the stratified sample), the data have been reweighted to estimate simple statistics (e.g. proportions in work or average wages). In Stata, analytic weights are used, [aweight=INF], which assign observations for those with fathers in manual occupations at birth a weight four times greater than those with fathers in non-manual and agricultural occupations.

- To improve the quality of imputations in the propensity-score models used for the analysis in chapter 5 and 7, father's social class has been included as a covariate in the model (in binary form, taking the values 1 and 0, rather than 1 and 4); and

- The weighting variable has been included as a probability weight (the inverse of the probability of an observation being selected into the sample) in the wage models used in the analysis in chapter 7 to adjust for the combined effects of stratification on sample estimates and standard errors. In Stata, probability weights [pweight=INF] are used.

4.3.2 Truncation of work experience measures for the 1946 cohort

Labour market experience is measured as the number of years in work since age 25 for the 1946 cohort, rather than since leaving full-time education. Up to ten years potential labour market experience are therefore not accounted for.[9] Labour market experience is used as a covariate in the following ways: 1) to predict potential hourly earnings for non-workers in the analysis presented in chapter 5; and 2) to account for gender differences in hourly earnings in the analysis presented in chapter 7. The truncation of employment experience may bias estimates from these two analyses, particularly estimates for the age 26 sample for whom only one year of labour market history is measured. This section uses a derived variable containing information on labour market experience from 18-25, available in the existing datasets, to assess the extent and likely effects of the truncation of employment experience.

A derived variable (NOWK1825) contains information on the number of months not working from 18 to 25, excluding months seeking work. The exclusion of time spent unemployed and seeking work from this measure means that it is likely to be an under-estimate of men's non-employment over this period. The variable contains information for 4,001 cohort members. Figure 4.4 shows gender differences in the distribution of this variable. For men, the mean amount of time spent out of work is nearly 12 months (a year), whereas the mean for women is close to 29 months (2 $\frac{1}{2}$ years). More than half of men spent no time out of work (and not looking for work) between the ages of 18 and 25.

Table 4.12 shows gender differences in mean employment experience over different ages. The greatest measured difference is between the ages of 25 and 31, over which period the majority of women in the cohort spent several years out of work raising children. Gender differences in early work experience (*7 years - years not working aged 18-25*) are smaller than gender differences in experience at later ages.

The analyses presented in chapters 5 and 7 were also run including the estimate of 18-25 experience as a covariate in the models. For the analysis in chapter 5, the

[9] For the 1958 and 1970 cohorts, the measures are based on recall data which have their own problems, not explored here. For example, Gregg (2001) found that past experiences of unemployment appeared to be underestimated by men in the 1958 cohort, comparing the recall data to official data for the corresponding period and age group. Short episodes occurring some years before the interview were more likely to be forgotten.

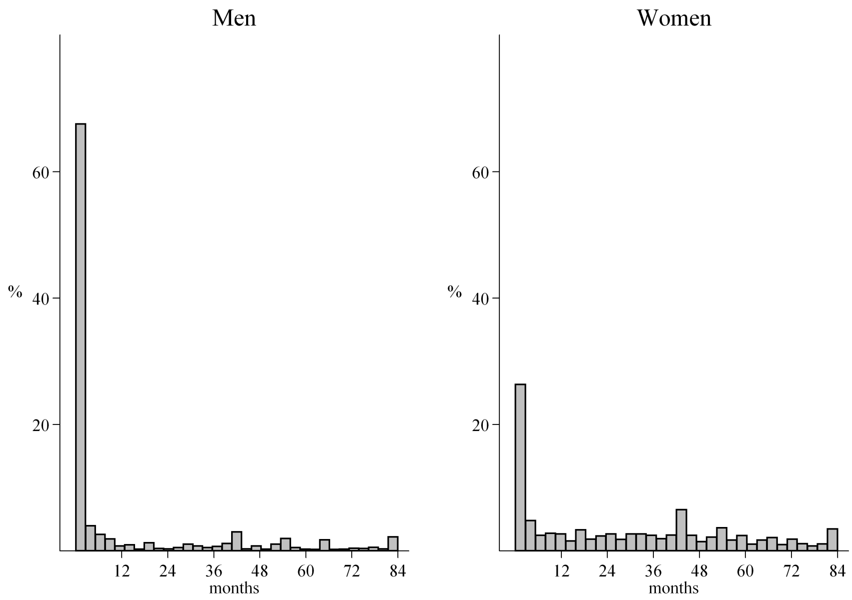

Figure 4.4: Months not working aged 18-25 for 1946 cohort, by sex

Table 4.12: Gender differences in mean employment experience over the life-cycle, 1946 cohort

	Years in work			Gender difference		
Ages	Potential	Women	Men	Abs.	Std. (1)	Sample size
18-25 (2)	7	4.66	6.29	-1.62	-26.6	4,001
25-31	6	3.41	5.78	-2.37	-36.8	3,301
31-36	5	2.85	4.64	-1.79	-30.3	3,178
36-43	7	5.86	6.64	-0.77	-12.3	3,248
Restricted sample (3)						
18-25 (2)	7	-	-	-	-25.9	3,700
25-26	1	-	-	-	-39.6	3,700

All figures are weighted to account for stratification.
(1) The standardised difference = (difference)/(standard error of diffference) i.e. the t-statistic.
(2) Exp (years) = (7 - NOWK1825).
(3) Sample is restricted to those with observed values for experience for ages 18-25 and 25-26

variable was included in the probit models used to generate a propensity score. The results were unchanged and are not shown. For the analysis presented in chapter 7, the point estimate of unequal treatment were slightly smaller for the age 26 sample of the 1946 cohort, but the difference in estimates was not statistically significant.

4.3.3 Combined effects of attrition and survey non-response

The main analyses in this thesis use cross-section samples that exclude cohort members who did not respond to a survey and those with missing values for highest qualification, employment experience or, for women only, presence of children in the household. Missing values for employment experience arise, in the main, from non-response to a previous survey in which information on job and unemployment histories was collected. The combined effects of attrition, non-response and missing educational or employment history data on the representativeness of the samples are assessed using information on early achievement in maths tests taken at age 10 or 11 by each cohort.[10] The likely bias in wage estimates from these samples is assessed by comparing estimates of median wages and female-to-male wage ratios with those from cross-sections of the Family Expenditure Survey for the same years.

Scores from mathematics tests taken at age ten or eleven are strongly correlated with family background characteristics, and positively correlated with future earnings and with the probability of remaining in the studies. As such, they are useful summary indicators of the extent of selectivity biases in samples induced by non-random attrition and survey non-response. The maths scores were were standardised to have mean zero and standard deviation 1 for the sample of boys and girls who took the test. There were small gender differences in scores at this age, with boys in the 1958 and 1970 cohorts tending to do slightly better than girls (table 4.13).

Table 4.13: Means of standardised maths scores taken at age 10 or 11, by sex and cohort

Cohort	Sample size	Mean score (std. deviation)	
		Girls	Boys
1946 cohort	4,025	+0.02 (0.97)	-0.02 (1.03)
1958 cohort	14,127	-0.02 (0.97)	+0.02 (1.02)
1970 cohort	11,685	-0.05 (0.96)	+0.04 (1.04)

Each score is standardised using the formula $x_i - \bar{x}/sd(x)$. The mean of the standardised distribution for the whole sample who took the test (boys and girls) is zero and the standard deviation is 1. The sample excludes children with either did not take the test or did not have a completely observed score.

[10]The effects of attrition, survey non-response and missing data arising from previous non-response were also analysed separately.

Changes in the sample participating in the cohort study over time show up as changes in sample means of maths scores. Figure 4.5 shows that 1946 cohort members with lower maths scores at age 11 were more likely to have left the study by age 31 and 43 and that the cumulative effects of non-response and attrition amongst those with lower scores increased between these ages. Positive selection into the sample appears to have been stronger amongst men than amongst women (i.e. men with lower scores were more likely than women with lower scores to leave the study). Figure 4.6 shows that 1958 cohort members with systematically lower maths scores at age 11 have also been more likely to have left the study by the age of 23. Again, the effects of selection have been slightly stronger for men than for women. Most strikingly, figure 4.7 shows strong positive selection into the sample that responded to the postal survey at age 26 (in 1996) and positive selection, although weaker, into both the age 30 and age 34 samples. The stronger selection bias in the age 26 sample of the 1970 cohort also shows up in wage differentials later on. The hourly wages, at ages 30 and 34, of cohort members who did not respond to the postal survey were between 8 and 12 per cent lower than those of cohort members who did take part in the postal survey.

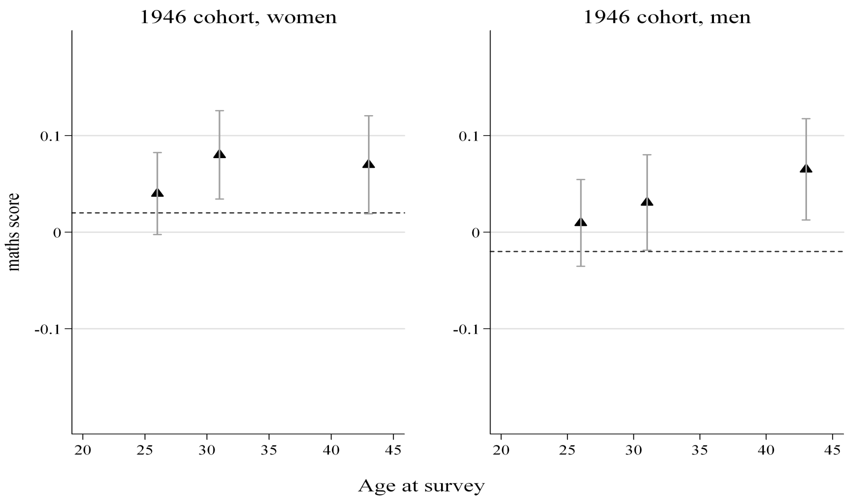

Figure 4.5: Mean standardised maths score at each survey of 1946 cohort, by gender

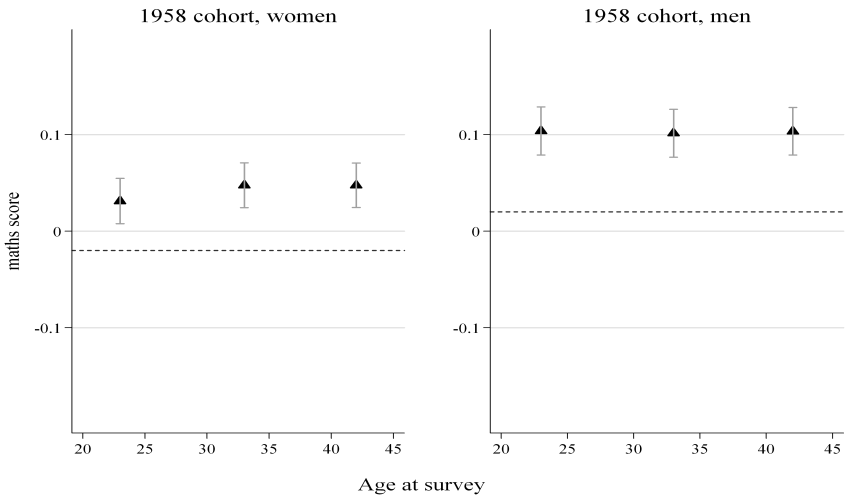

Figure 4.6: Mean standardised maths score at each survey of 1958 cohort, by gender

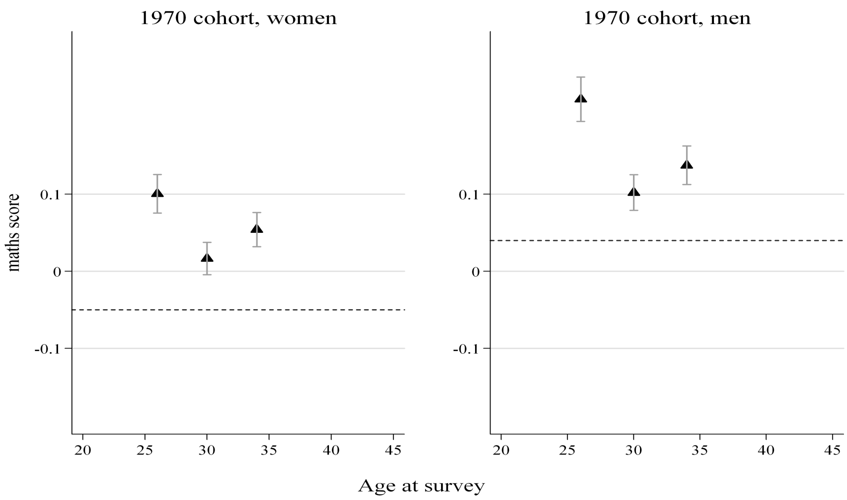

Figure 4.7: Mean standardised maths score at each survey of 1970 cohort, by gender

Table 4.14 compares estimates of median hourly wages and female-to-male ratios from the cohort samples with those from samples from the Family Expenditure Survey (FES) carried out in the same years. The FES samples include individuals of a similar age to the cohort members. The suggested patterns of bias are consistent with the pattern observed in maths scores. The effects of positive selection into the studies appears to increase with age. By age 43, the effects of positive selection amongst men into the sample appears to bias upward slightly the estimated female-to-male median wage ratio. The effects of positive selection into the 1958 cohort samples also bias upwards the estimates of the female-to-male median wage ratio by ages 33 and 42. For the 1970 cohort, the estimated ratio is biased upward at age 26, reflecting strong positive selection into the sample that responded to the postal survey at this age, plus stronger selection amongst men than women. The estimates at ages 30 and 34 are not significantly different from the FES estimates - the confidence intervals around the FES estimates are quite wide for these samples.

Table 4.14: Comparison of median wages and female to male wage ratios in the cohort surveys and the Family Expenditure Survey

Survey year and contact age (1)	Sample size (2)		Median wage		Female-to-male ratio	
	Cohort	*FES*	*Cohort*	*FES*	*Cohort*	*FES*
1946 cohort (3)						
1972, age 26	2,171	592	5.38 (5.30, 5.47)	5.28 (5.07, 5.50)	0.70 (0.66, 0.73)	0.68 (0.63, 0.74)
1977, age 31	1,796	663	5.60 (5.46, 5.75)	5.63 (5.45, 5.82)	0.63 (0.61, 0.67)	0.65 (0.61, 0.69)
1989, age 43	2,184	605	6.95 (6.68, 7.21)	6.92 (6.39, 7.44)	0.61 (0.57, 0.65)	0.53 (0.48, 0.58)
1958 cohort						
1981, age 23	7,946	572	5.20 (5.15, 5.24)	5.22 (5.06, 5.38)	0.84 (0.83, 0.86)	0.79 (0.73, 0.85)
1991, age 33	6,802	535	7.57 (7.45, 7.68)	7.10 (6.77, 7.41)	0.70 (0.68, 0.72)	0.63 (0.56, 0.70)
2000, age 42	7,250	494	7.90 (7.78, 8.02)	8.22 (7.67, 8.77)	0.67 (0.65, 0.69)	0.58 (0.51, 0.65)
1970 cohort						
1996, age 26	5,972	450	6.69 (6.59, 6.78)	6.73 (6.37, 7.09)	0.90 (0.87, 0.92)	0.81 (0.73, 0.89)
2000, age 30	7,393	542	7.57 (7.49, 7.66)	7.82 (7.38, 8.33)	0.86 (0.84, 0.89)	0.81 (0.71, 0.92)
2004, age 34	6,206	533	8.68 (8.56, 8.79)	8.70 (8.20, 9.20)	0.80 (0.78, 0.83)	0.87 (0.77, 0.97)

(1) The FES samples include individuals either the same age, one year older or one year younger than the birth cohort samples (2) Samples include employees with an observed hourly wage, excluding values more than £100 and less £2 an hour. (3) Figures for the 1946 cohort are weighted.

The picture of selectivity bias induced by non-participation in the studies is consistent across internal (maths scores) and external checks (FES wages). In terms of bias in the present analyses, the results suggest that women's relative pay may be overstated at older ages in both the 1946 and the 1958 cohorts. In the 1970 cohort, women's relative pay is likely to be overstated at age 26.

There are a number of ways of handling non-random non-response and missing data. One method is to use sample weights containing information on the probability of responding (or not responding) based on complete earlier data. Universal weights are not provided with the cohort datasets, unlike the BHPS for example, which provides several different sets of weights. Hawkes and Plewis (2006) suggested that the application of universal weights could, in any case, increase rather than decrease bias in the case of the 1958 cohort. They point out that patterns of wave non-response, attrition and lacking education data differ and that different substantive models may require different sets of weights. Weights were not derived for the present analysis simply because of the additional work involved in specifying a sound model to predict non-response weights.

Another option is imputation of values for missing items. Mean imputation has in fact been used for some covariates in the probit models in chapter 5 (i.e. the childhood background characteristics and maths scores). Mean imputation is a fairly crude approach and is not viewed as a good way to impute missing covariates. In practice though, the predictions from these models were not sensitive to the inclusion of childhood variables, once qualifications, parental status and employment characteristics were included. On this basis, more sophisticated imputation models were not pursued. Missing educational qualifications and employment experience were not imputed, again because of the complexity of the datasets and the additional work required to specify a good imputation model.

Both weighting and imputation models use the assumption that the data are missing at random (MAR) - that is missing at random, conditional on the observed data, as opposed to missing completely at random (MCAR). If there is a strong possibility that there are unobserved sources of selectivity bias, heckman-type methods may be more suitable. These were described in the last chapter with application to employment participation and the application to survey response is similar. **?** used Heckman (1979) to adjust for unobserved differences in occupational attainment across respondents and non-respondents to the 1981 (age 23) survey of the 1958 cohort. They also tested the stability of estimates to the exclusion of variables with a lot of missing items and used occupation of first job as an additional covariate to minimise unobserved fixed individual effects.

A final approach is to test the robustness of important estimates to variation in the sample and variable selections that are most likely cause bias. Similar to **?**, the internal consistency of the results can be checked in a pragmatic way, by testing the stability of parameters to different models. This was the approach taken in the present analysis:

- Based on the evidence that relatively low response rates to the postal survey of the 1970 cohort in 1996 (age 26) did affect the representativeness of the sample at this age, the analysis was carried out both for the cross-sections who took part in any one of the adult surveys, and also for the sub-sample who took part in all three of the adult surveys (not presented). Our conclusions were not altered.

- To test the effects of increasing positive sample selection on estimated life-cycle trends, the same estimates were made for longitudinal samples. This deals with internal consistency, but not external consistency i.e. we can be sure that the trends have not arisen from attrition, but not that they are generalisable to a broader population.

4.4 Conclusions

The birth cohort data offer critical advantages for the present analysis. Detailed information on job and unemployment histories and childbearing histories are important for the analysis of gender wage differentials. The longevity and longitudinal aspects of the datasets are also valuable for analysing trends over an important period of British history and for looking at life-cycle trends. However, the data are difficult to use and a lot of work was put into identifying the appropriate original variables, checking and recoding the data and deriving suitable variables for analysis. The derived work experience and hourly earnings variables have been contributed to the NSHD data access system. Derived variables for the NCDS and BCS70 will be contributed to the CLS collection.

Some bias in wage estimates is caused by the combined effects of attrition, survey non-response and question non-response. The samples have become less representative over time for the 1946 and 1958 samples and the 1996 (age 26) postal survey of the 1970 cohort had a lower response rate and non-random patterns of response. As a consequence, there is slight upward bias in estimates of women's relative pay for the 1946 and 1958 cohorts at older ages and for the 1970 cohort at age 26. The approach taken in the analyses in this thesis has been to test the sensitivity of the main substantive conclusions to different sample and variable selections, including longitudinal samples.

Appendix 4: Questions asked at each survey

Table 4.15: Current economic status questions at each survey - 1946 birth cohort

Year	Age	Employment status questions	Employment status categories	Full-time/part-time
1972	26	Are you now in paid work? Are you self-employed?		Is this work full time or part time? Do you work on average for as much as 8 hours a week?
		What are you doing at the moment?	unemployed, full time student, full time housewife, other	
1977	31	Are you now...	...working full-time, working part-time, a full-time housewife, unemployed, or doing something else?	
		Are you self-employed?		Full or part-time?
1989	43	Are you in paid work now? Please describe the job. Are you...	...an employee, self-employed	Do you work full-time (30 hrs or more) or part-time?
		Are you not in paid work because of health problems? During this time were you a student or on a training course?		
		What is your occupation?...	looking after the home, no other occupation, other (specify)	

Table 4.16: Current economic status questions at each survey - 1958 birth cohort

Year	Age	Employment status questions	Employment status categories	Full-time/part-time
1981	23	Are you...	... an employee, working as a temp for an agency, self-employed?	Do you think of yourself as working full-time or part-time? *(only asked if reported average weekly hours was less than 30 or varied too much to say)*
		Are you registered with either a jobcentre or Government employment office as looking for work? Are you currently taking a course for any qualifications?		
		Is respondent currently out of the labour force? What is respondent currently doing?	housework, prison/borstal, unable to work/sick/disabled, extended holiday, other	
1991	33	Which of the theings on this list describes what you are currently doing? *code one only - main activity*	full-time paid employee (30+ hours a week), part-time paid employee (under 30 hours a week), full time self-employed, part-time self-employed, unemployed and seeking work, full-time education, temporarily sick/disabled (up to 6 months), permanently sick/disabled, looking after home/family, other (please specify)	Do you work 30 or more hours a week in this job? *only include hours on call if they are paid for, teachers: term time hours*
2000	42	Which of the things on this card best describes what you are currently doing? *code one only - main activity*	full-time paid employee (30+ hours a week), part-time paid employee (under 30 hours a week), full time self-employed, part-time self-employed, unemployed and seeking work, full-time education, on a government scheme for employment training, temporarily sick/disabled (up to 6 months), permanently sick/disabled, looking after home/family, wholly retired, other (please specify)	

Table 4.17: Current economic status questions at each survey - 1970 birth cohort

Year	**Age**	**Employment status questions**	**Employment status categories**
1996	26	Which of the following best describes what you are currently doing?	full-time paid employee (30 or more hours a week), part-time paid employee (under 30 hours a week), full-time self employed, part-time self employed, unemployed and seeking work, full-time education, temporarily sick/disabled (less than 6 months), long-term sick/disabled (6 months or longer), looking after home/family, on a training scheme, something else
2000	30	Which of the things on this card best describes what you are currently doing? *code one only - main activity*	full-time paid employee (30+ hours a week), part-time paid employee (under 30 hours a week), full time self-employed, part-time self-employed, unemployed and seeking work, full-time education, on a government scheme for employment training, temporarily sick/disabled (up to 6 months), permanently sick/disabled, looking after home/family, wholly retired, other (please specify)
2004	34	Which of the things on this card best describes what you are currently doing? *code one main activity only*	full-time paid employee (30+ hours a week), part-time paid employee (under 30 hours a week), full-time self-employed, part-time self-employed, unemployed and seeking work, full-time education, on a government scheme for employment training, temporarily sick/disabled (¡6 months), permanently sick/disabled (6+ months), looking after home/family, wholly retired, other (specify at next question)

Table 4.18: Wage questions at each survey (referring to main job only) - 1946 birth cohort

Year	**Age**	**Pay question**	**Pay period**	**Hours question**
1972	26	'Including all regular payments such as overtime, bonuses etc..., how much do you earn in a typical week or month before deductions for tax, national insurance etc..?'	(per week, per calendar month, per four weeks, other)	'how many hours would you say you work in a typical week, excluding overtime?'
		'In your job is there a basic or standard rate of pay? If yes, how much is this, before deductions for tax, national insurance etc...'		'Do you ever work paid overtime hours?'
				'how much overtime do you work regularly and how much occasionally?'
1977	31	'On average, how much do you earn a week? (including overtime and other payment) before deductions'		'How many hours a week do you usually work including overtime?'
1989	43	'Would you mind telling me which of the letters on this card represents your own average gross earnings, before deduction of income tax and national insurance?'	26 wage bands shown in anuual, monthly and weekly amounts	'How many hours a week on average do you have to work to earn this amount?'
				'How many months a year on average do you have to work to earn this amount? (if in part-time or seasonal work)'
				'Last week (or last full working week) how many hours did you actively spend working including overtime and working at home?'

Table 4.18: Wage questions at each survey (referring to main job only) - continued - 1958 birth cohort

Year	Age	Pay question	Pay period	Hours question
1981	23	'On the last occasion what was your pay before deductions for tax and National Insurance: including any overtime, bonus, commission, tips? (if last occasion was usual amount)'	per day, per week, per 2 weeks, per month, per 3 months, per 6 months, per year, other	'How many hours of paid work do you actually do in an average week - including any paid overtime you usually do, but excluding meal breaks?'
		'And what is your usual pay before any deductions for tax and National Insurance: including any overtime, bonus, commission, tips, etc., that you usually receive? (if last pay was unusual)'		
1991	33	'What is your usual gross pay before deductions?'	'How long a period does that pay cover?' (1 week, fortnight, four weeks, calendar month, year, other)	'How many hours a week do you usually work for that pay, excluding meal breaks but including paid overtime?'
		'Last time you were paid, what was your gross pay before deductions?' (including overtime, bonuses, commission and tips)'		
2000	42	'Last time you were paid, what was your gross pay before deductions?' (including overtime, bonuses, commission and tips)	as above	'(still thinking of your main job) Do you ever do any work which you would regard as paid or unpaid overtime?'
				'How many hours per week do you usually work in your (main) job/business not including meal breaks? (if no overtime)'
				'How many hours a week do you usually work not including meal breaks and overtime (if overtime)'
				'How many hours paid overtime do you usually work per week?'

Table 4.18: Wage questions at each survey (referring to main job only) - continued - 1970 birth cohort

Year	Age	Pay question	Pay period	Hours question
1996	26	‘What is your usual take home pay (after deductions, but including any bonuses or overtime)? Please write in amount’	‘tick one box for period covered’ (hour, day, week, month, year, other period)	‘How many hours do you usually work each week? Please include any paid overtime you usually do, but exclude meal breaks’
2000	30	‘Last time you were paid, what was your gross pay before deductions? (including overtime, bonuses, commission and tips)’	‘How long a period does that pay cover?’	‘(still thinking of your main job) Do you ever do any work which you would regard as paid or unpaid overtime?’
			(1 week, fortnight, four weeks, calendar month, year, some other period)	‘How many hours per week do you usually work in your (main) job/business not including meal breaks? (if no overtime)’
				‘How many hours a week do you usually work not including meal breaks and overtime (if overtime)’
				‘How many hours paid overtime do you usually work per week?’
2004	34	as above	as above	as above

Table 4.19: Employment history questions at each survey, 1946 birth cohort

Survey year	Main question/instructions	Detailed questions
1972	Could you tell us about all the jobs, including promotions or changes within the firm, that you have done since your 25th birthday? Could you start with the job, if any, on your 25th birthday.	When did you start? Was it full time or part time? When did you leave?
	Could I now ask you some more details about the periods when you have not been in paid work for a week or more for any reason since your 25th birthday (asked if relevant)	What were you doing in this period? When did this start? When did this end? Did you register as unemployed?
1977	The last job you told us about is written in red below. Please give details of all jobs (including promotions or changes within the firm) you have had since then, putting in any periods you have had off work (e.g. as a housewife or student). If there have been no changes at all, put 'still there'.	Full or part time? Date started month, year? Date left month, year?
	Have you had a long spell (a month or more) off work through illness since March 1972? Have you had a long spell (a month or more) off work because you were unemployed and looking for a job since March 1972?	Date started (month, year), Time off work/Length of time unemployed
1982	The last job you told us about was in [ ]. Since then have you changed your job? Could you tell me about all the paying jobs you've had since then, not including your present job. *Interviewer - begin with last job before present job (or spell out of work) and work backwards in order, to the job immediately following our last record.*. Up to 7 jobs.	Was it full-time (30 hours or more a week) or part-time? When did you start this job? (month, year) When did you leave? (month, year)
	Have you had any periods of a month or more when you weren't in any kind of paid work (include full-time or part-time) since [ ]? *Interviewer - begin with the present or last period and work backwards in order.* Up to 4 spells.	When did this start? (month, year) When did it end?
1989	Job held in 1982 (*only for those not visited in 1982*). Current or most recent job. Longest job since 1982.	Do you/did you work full-time (30 hours or more) or part-time? When did you begin this job? (month year) When did you end this job?
	Going back to 1982 have you had any spells of a month or more when you were not in any kind of paid work? *Begin with the present spell (if applicable) and work backwards in order.* Up to 6 spells.	When did you begin this spell? (month, year) When did it end? And so it last for (weeks)?

Table 4.20: Employment history questions at each survey, 1958 birth cohort

Year	Age	Main questions	Detailed instructions
1981	23	Has the respondent had any jobs since leaving school? Total number of jobs which lasted one month or more excluding vacation jobs.	A job is a period of time with the same employer or a period self-employed. Do not include: holiday jobs while at school or in full-time education; jobs which did not last for at least one month; part-time jobs done at the same time as full-time education; part-time jobs done at the same time as a full-time job. The job is a period spent working as a temp for one or more agencies not each employer worked for on a temporary basis.
		Can I just check some details about the jobs you have done. By a job I mean a period of time with the same employer even though you may have done different work during that period or, if you were self-employed, a period of time doing the same self-employed work.	Collect details of up to four jobs. If respondent has had five or more jobs, collect first three and latest one. Check job was... full-time (30 hrs or more) or part-time (less than 30 hrs)
		Has the respondent had any periods of unemployment since leaving school? Unemployment means being out of work and wanting work. You do not have to be registered as unemployed. Do not include holidays or vacations while in full-time education.	Collect details of up to four periods of unemployment. If respondent has had five or more, collect first three and latest period.

Table 4.20: Employment history questions at each survey, 1958 birth cohort - continued

Year	Age	Main questions	Detailed questions/instructions
1991	33	Please give details of each paid job you have done which lasted at least a month, by answering questions (a)-(f). Please start with your first job and work forwards to your current or last job. Was job full-time (30+ hours) or part-time (<30 hours)?	Include any job, full-time or part-time, which you did for at least a month. If you changed the kind of work you did while working for an employer, count this as still the same job. Only a change of employer counts as a change of job. If you have worked for a Government Department, school or hospital, count as a change of job any change of Government Department, school or hospital. If you had a period of "temping", or free-lancing, or consultancy, or self-employed contract work, count the whole period as one job. Include work in sheltered workshops. Don't count work experience, sandwich jobs or holiday jobs while you were in full-time education. If you went on maternity leave or sick leave and went back to the same job, count the whole period as one job. Don't count time spent on a Government work or training scheme.
		Since leaving school has there been any period of a month or more when you did not have a paid job and when your situation was best described by one of the categories below. (unemployed and seeking work, Government training or work scheme, full-time education, full-time housework or childcare, unable to work because of sickness or handicap, other).	If you were on maternity leave or sick leave from a job, and then went back to the same job, do not count that as time not in a job
2000	42	We have talked about what you are currently doing. Now I want to gather a few details about the jobs and other things that you may have been doing since March 1991. You said you started your previous period of [*last recorded activity*] in [*month*] of [*year*]. Which of the things on this card best describes what you were doing before this period of [*last recorded activity*]? (1).	Check that respondent has been doing this continuously since the start date and that there has been no time when the situation changed.

(1) The list of activities is the same as for the question on current main activity - see table 4.16.

Table 4.21: Employment history questions at each survey, 1970 birth cohort

Year	**Age**	**Main questions**	**Detailed questions/instructions**
2000	30	We have talked about what you are currently doing. Now I want to gather a few details about the jobs and other things that you may have been doing since March 1986. You said you started your previous period of [*last recorded activity*] in [*month*] of [*year*]. Which of the things on this card best describes what you were doing before this period of [*last recorded activity*]? (1).	Check that respondent has been doing this continuously since the start date and that there has been no time when the situation changed.
2004	34	We have talked about what you are currently doing. Now I want to gather a few details about the jobs and other things that you may have been doing since [*Date of last interview/you were age 16*]. You said you started your previous period of [*current/previous*] period of [*activity*] in [*month* and *year* when started activity]. Which of the things on this card best describes what you were doing before this period of [*activity*]? (1).	Check that respondent has been doing this continuously since the start date and that there has been no time when the situation changed. Code only one main activity. (Glossary of definitions).

(1) The list of activities is the same as for the question on current main activity - see table 4.17.

Chapter 5

Analysis I: Trends in women's and men's relative pay opportunities

This chapter analyses changes in the pay opportunities of women and men across the three British birth cohorts over the period 1972-2004. The analysis of earnings is extended to take into account the potential earnings of those not in paid work. Potential hourly earnings are imputed for each non-employee using the actual hourly earnings of an otherwise-similar employed individual, based on nearest-neighbour matching. The results suggest that the large fraction of women not in paid work in the 1970s had low average potential earnings. Consequently, the cross-cohort increase in women's pay opportunities is estimated to be greater than the increase in the pay of employed women, both in real terms and relative to men's.

The chapter is organised into four sections. The first section summarises the datasets used and trends in employment and pay across the three cohorts. The second section describes the methods used to impute potential wages for non-working individuals. The third section presents the main results of the imputations, including results from tests to assess the validity of the model assumptions. The final section discusses the results.

5.1 Data and description of trends

Tables 5.1 and 5.2 show the percentage of women and men in each main economic activity category for each of the survey samples used. Potential wages are imputed for individuals who are either not employed, who are self-employed or who are employed, but did not respond to the questions about earnings. For women, most of those who are not employees are full-time housewives. For men, a substantial fraction are self-employed at later ages, whilst a significant minority are unemployed in the 1958 cohort at age twenty-three (in 1981). The unemployment category includes registered and unregistered unemployment, plus those enrolled on a Government training scheme.

Table 5.1: Percentage of women in each economic activity category, by cohort and age

	1946 cohort			*1958 cohort*			*1970 cohort*		
Age at survey	*26*	*31*	*43*	*23*	*33*	*42*	*26*	*30*	*34*
Full-time employee	31	21	37	56	33	40	62	48	40
Part-time employee	13	29	34	6	29	31	12	21	29
Full-time self-employed	1	3	6	1	4	4	3	3	3
Part-time self-employed	1	2	2	·	3	3	1	2	3
Employed, hours unknown	·	·	1	1	·	·	·	·	·
Full-time housewife/carer	51	45	11	24	27	13	14	20	19
Unemployed	1	1	4	7	2	2	2	2	2
Not in work, ill or disabled	·	·	3	·	1	5	2	2	2
Full-time student	1	1	1	2	1	1	2	1	1
Other	·	·	·	2	1	1	2	1	2
Sample size (1)	1,852	1,649	1,618	6,256	5,785	5,777	4,835	5,766	5,025

(1) The samples include all cohort members who participated in the survey and who answered the question on current economic activity. Individuals engaged in more than one economic activity count only once, based on their self-reported main economic activity. Percentages are rounded up or down to nearest integer are not given when less than half a percent of the sample fell into the specified category. Percentages for 1946 cohort are weighted to account for the stratification of the sample.

The changes in women's employment and pay across the cohorts is striking. In 1972, just under half of women from the 1946 cohort were in paid employment at the age of twenty-six. Nearly quarter of a century later, more than three-quarters of women from the 1970 cohort were in paid work at the same age. Across these samples, women's relative median pay increased from 70 per cent to 90 per cent of men's.

The proportion of women who are in paid employment, when surveyed in their twenties and thirties, has increased across each successive cohort. This is illustrated in Figure 5.1.[1]

[1] The figures in this section are all based on data from the cross-sections of cohort members who participated in the surveys. Figures for the 1946 data are weighted to adjust for stratification.

Table 5.2: Percentage of men in each economic activity category, by cohort and age

	1946 cohort			*1958 cohort*			*1970 cohort*		
Age at survey	*26*	*31*	*43*	*23*	*33*	*42*	*26*	*30*	*34*
Full-time employee	84	83	70	75	74	71	75	77	77
Part-time employee	·	·	1	1	1	1	2	1	1
Full-time self-employed	10	12	21	6	16	17	10	11	14
Part-time self-employed	·	·	·	·	·	1	1	1	1
Employed, hours unknown	·	·	1	1	·	·	·	·	·
Full-time carer	·	·	·	·	·	1	·	1	1
Unemployed	3	4	2	12	6	3	7	5	3
Not in work, ill or disabled	1	·	4	·	2	5	2	3	3
Full-time student	1	1	·	3	·	·	4	1	1
Other	·	·	·	2	1	1	1	1	1
Sample size (1)	1,897	1,661	1,607	6,249	5,582	5,605	4,063	5,436	4,609

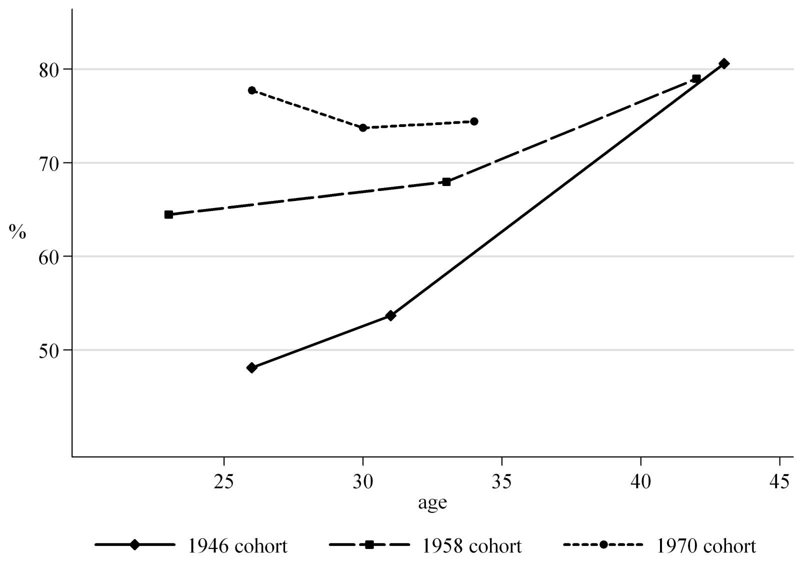

Figure 5.1: Percentage of women in work at each age, by cohort (including self-employees)

The cross-cohort increase in women's rates of employment is composed both of an increase in the proportion of women not having children or having them later, and an increase in rates of employment amongst mothers of young children. Figure 5.2 shows the percentage of women in each cohort who had become mothers by each age for the three cohorts.[2] The flatter slopes for the two later cohorts show that years of

[2]The figure uses birth history data collected from all mothers and is based on the sample interviewed at age 43 for the 1946 cohort, age 42 for the 1958 cohort and age 34 for the 1970 cohort.

childbearing have become more spread out, as well as later.

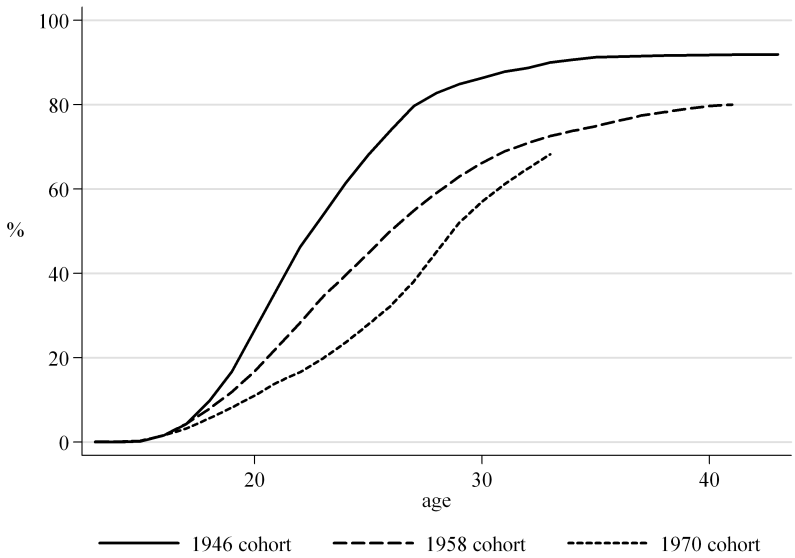

Figure 5.2: Percentage of women who have had a first child by each age, by cohort (live births only)

The length of time spent out of work amongst women who have children has also decreased across the three cohorts. For the 1946 cohort, only a fifth returned to work within a year of a first birth, compared to nearly two-fifths for the 1958 cohort and nearly 60 per cent for the 1970 cohort (for a fuller analysis, see Macran et al., 1996; Smeaton, 2006).

Among men, in contrast to women, rates of employment have decreased slightly across the three cohorts. The decrease is most marked across the 1946 and 1958 cohorts when in their twenties and thirties. This is illustrated in Figure 5.3. Both the 1958 and 1970 cohorts were affected by recessions at the start of their careers. The 1946 cohort were in their mid-thirties by the time of the 1980s recession.

Women's median hourly earnings have increased relative to men's across the three cohorts. This is illustrated in Figure 5.4. This shows an increase in women's relative pay across the cohorts (reading vertically across the lines). In the two later cohorts, it shows a decrease women's relative pay with age during their twenties and early thirties. [3]

[3]Women's and men's median real earnings both increase with age, but men's increase more than women's.

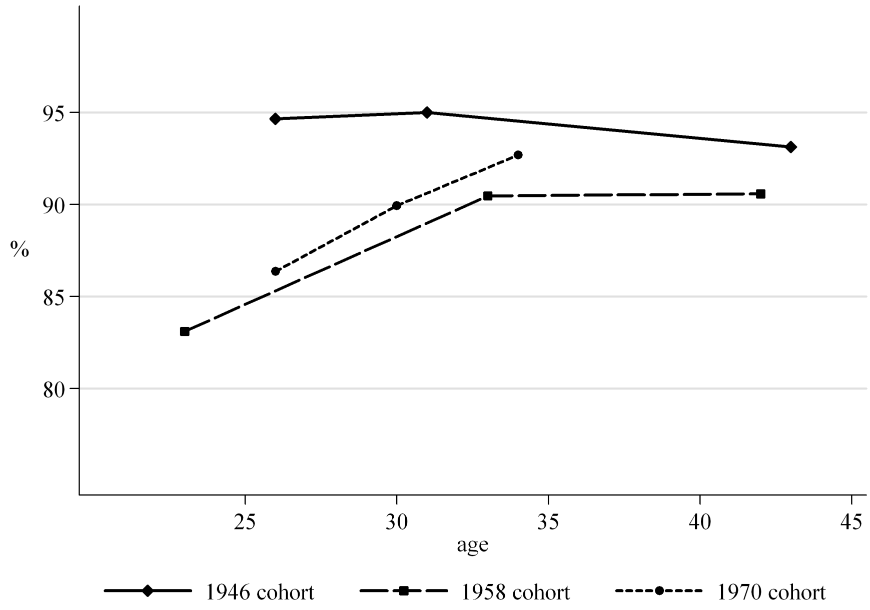

Figure 5.3: Percentage of men in work at each age (including self-employees)

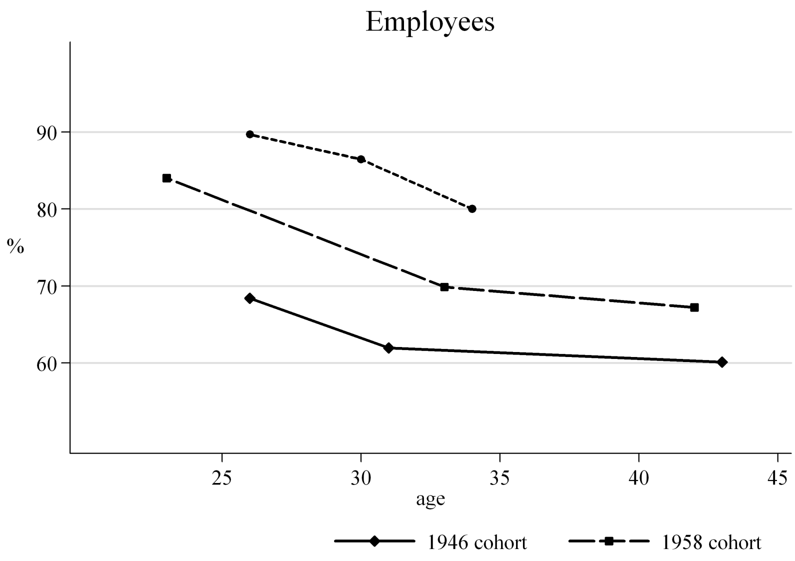

Figure 5.4: Women's median hourly earnings as a % of men's at each age, by cohort (employees only)

The purpose of the analysis presented in the rest of the chapter is to assess how well trends in the relative pay of men and women in work (figure 5.4) represent trends in the relative pay opportunities facing women and men in the whole population. The concern is that cross-cohort changes in patterns of life-cycle employment participation will have altered the relationship between actual and potential wage distributions. As a consequence, trends in relative median pay may misrepresent cross-cohort and within-cohort changes in women's and men's relative pay opportunities.

The approach taken here is to impute potential hourly wages for non-working individuals based on the wages of observably-similar working individuals. For employed individuals, the potential wage is taken to be the wage that they are currently earning.[4] For non-employed individuals, the potential wage is defined as the wage that they could expect to earn if they entered paid work. This is estimated as the actual wage of an employee born in the same year, of the same sex and the same age, with similar qualifications, childhood ability scores, social class and family background and a similar child-bearing and employment history. The large and detailed files of information on members of the British Birth Cohort Studies make these studies an ideal source of data for this statistical exercise. The approach used is similar to that used by Blau and Kahn (2006a) and Olivetti and Petrongolo (2008) to analyse gender gaps in employment and pay in the US and across OECD countries respectively.

The validity of the imputation exercise rests on the assumption that there is a meaningful existing distribution of potential wages for non-employed individuals at each point in time, over the period covered by the analysis. This would not be a valid assumption, for example, if there were complete cultural, legal or economic restrictions on employment for certain non-employed groups.[5] A further assumption is that the potential wage can be meaningfully defined in situations in which individuals may not be actively seeking work or may have only a vague expectation of what they could earn if they took a paid job.[6] The exercise here does not attempt to model the general equilibrium and other effects of women's (and men's) changing patterns of employment participation on wages, and vice versa. Rather, a more modest aim is to estimate the distribution of wage opportunities, as it exists, in a real but intangible form, alongside

[4]From a search-theoretic perspective, their current actual wage could be seen as just one in a range of potential wages that they could earn, depending upon their job search activity.

[5]In the extreme, if there were no paid employment opportunities for a particular group, the potential wages of individuals in that group would, by definition, be valued at zero.

[6]In the period covered by my analysis, most married women and nearly all men work for a large part of their adult lives. However, cultural norms about the employment of mothers with young children have changed over the period covered.

a given rate of employment and distribution of wages.

5.2 Model and variables used for imputing potential wages

The imputation exercise is aimed at estimating $E(w^o|g, s = 0)$, where w^o is the potential wage, g is an indicator of the age-cohort-sex specific group (for whom potential wages are separately imputed) and s is an indicator of employment status where $s = 0$ indicates non-employment (and $s = 1$ refers to employees). Potential wages are also imputed for self-employees and for employees who have missing wages. A significant fraction of men in each cohort are self-employed at each age. The discussion in chapter 3 referred to the problem of selectivity bias induced by non-employment, rather than by self-employment or non-response to questions on earnings. This section separately considers imputation for self-employees and employees with missing wages.

The imputation methods I have used assign each non-employee (including self-employees) and employees with missing wages a potential hourly wage using data on the actual wages of employees who are similar to them across a range of selected characteristics and whose wages are observed.[7] The assumption is that the selected characteristics capture the important influences on the potential wage. These characteristics may have a direct and/or an indirect effect on the employment decision. Any residual correlation between the employment decision and the potential wage, i.e. differences in the potential wage across employed and non-employed groups with the same characteristics, is assumed to be small and negligible. The general assumptions upon which this 'imputation on observables' strategy is based were discussed in chapter 3 (section 3.2.1).

It is the ratio of women's to men's median hourly wage that is the summary statistic used as the measure of relative wages. The ratio of women's to men's potential wage, including imputed values for non-workers, is the statistic used to measure relative wage opportunities. The decision was to focus on median wages, rather than the mean or, if log wages were used, the geometric mean. In practice, the substantive conclusions presented in the chapter were the same when relative mean wages (and mean log wages) were used. However, comparing medians means that less reliance is placed on the goodness of the imputed value - the median is stable as long as the value imputed is on the correct side of the median. Further, within-cohort and cross-cohort comparisons of relative medians, rather than means, place less weight on changes in the effects of

[7] I have not used wage data collected in past surveys to impute potential wages for formerly-employed individuals who are not in work. This is partly because the data collections were between four and ten years apart, but mainly because the life-cycle dimension of wage dynamics is itself a focus of interest.

the widening wage distribution and the associated increasing distance between women's wages at the bottom and men's wages at the top of the distribution.

Standard errors using bootstrap methods, rather than analytically, since there is no simple formula for the standard error of the ratio of two medians. This involved drawing repeated, random sub-samples from the imputed wage samples, calculating the median (or other percentile) for each sub-sample and combining these sub-sample estimates to calculate the standard deviation in the medians (or other percentile), which is then used as the estimate of the standard error. The size of each of these repeated sub-samples was restricted to the number of observations in the original non-imputed wage sample.

5.2.1 Propensity score matching vs. regression methods

The assumption of selection on observables is not sufficient on its own to impute potential wages. I have experimented with different forms of imputation, which rely upon different identifying assumptions about the structural forms of the relationships between employment, potential wages and characteristics. The results presented in the main body of this chapter are from a model which uses nearest-neighbour imputation based on the propensity score.

Regression methods were also used to model the effects of the selected characteristics on log wages for employees and to predict log wages for non-employees using this model. In fact, the results from this model were substantively similar to those obtained from propensity score matching.[8]

The results from the propensity score model were preferred for two reasons:

1. Tests of the fit of the least-squares regression models (e.g. tests for heteroskedasiticity of residuals) suggested that the functional form was not well-specified in all cases. The wage predictions from the regression model are reliant on the functional form.

2. The Stata program psmatch2 (Leuven and Sianesi (2003)) includes some useful built-in features (under the command pstest) for examining differences in characteristics across matched groups.

Propensity score matching also has an important disadvantage. Reducing reliance on functional form using propensity-score matching comes at the cost of increasing

[8]Stata results are available on request. In chapter 7, results vary by method and each is shown.

reliance on the validity of a relatively small number of observations, when matching is performed with replacement. In other words, for non-working women with low wage characteristics, there may be only a small sample of working women who share similar characteristics. The wages of this small sample are used to impute wages for the non-working group. In contrast, in a regression model, the reliance is primarily on the functional form - potential wage predictions for the non-working group are based on an extrapolation of specified (log-linear) relationships between characteristics and wage levels observed for the working sample.

5.2.2 Nearest-neighbour imputation using propensity score

The imputation method used for the present analysis involves two stages. First, the degree of similarity and difference between employees and non-employees across a range of characteristics is reduced to a single propensity score. The propensity score is the predicted individual probability of being in paid employment. This score is calculated from a probit model using a set of observed characteristics to discriminate between the working population and the non-working population. In essence, the propensity-score gives greatest weight to those characteristics which discriminate most strongly between these two groups. Second, each non-employed individual is matched to an employee with the most similar propensity score. Each empty cell, representing the unobserved potential wage for a non-employee, is replaced with a value equal to the observed wage for a matched employee. Propensity score matching stems from the work by Rosenbaum and Rubin (1983) (see section 3.2.1 and equation 3.5 in chapter 3). Matching on the propensity score is less computationally intensive and more efficient than matching cell-by-cell across a large set of variables.

The parametric part of the method is the estimation of a probit model. The propensity score, p is a scalar value representing the probability of being in paid employment, estimated from a probit model separately for each gender, cohort and age, conditioning on some set of characteristics included in X. The estimated propensity score, $\hat{p}$, is given by:

$$\hat{p} = \Phi(X'\hat{\beta}) = P(s = 1|X) \tag{5.1}$$

where Φ, is the cumulative distribution function of the standard normal distribution and $\hat{\beta}$ is a vector of parameters estimated by maximum likelihood.

By construction, the majority of non-employees have lower propensity scores than employees and their corresponding employed wage donors have lower propensity scores than other employees. A necessary condition for propensity score matching is that the distributions of propensity scores for employees and non-employees overlap i.e. there are individuals who are working despite a low predicted probability of doing so.

The matching is carried out on a pair-by-pair basis. Given a sample of r employees and $n-r$ non-employees, the estimated propensity scores for the sample are $\hat{p_1}, ..., \hat{p_n}$. The wages for employees are observed as $w_1, ..., w_r$ whilst the potential wages for non-employees, $w_{r+1}, ..., w_n$, are unobserved. A potential wage is imputed for each non-employed individual, i, using the wage, w_i, of the most similar employed individual, j, where j is identified by the algorithm

$$|\hat{p_i} - \hat{p_j}| = \min_{1 \leq k \leq r} |\hat{p_i} - \hat{p_k}| \tag{5.2}$$

for any individual k amongst the employed sample.

5.2.3 Variables used

In selecting variables to include in the probit models, forming the basis for the propensity score, the following points were considered:

1. The aim is to cover the main joint influences on employment decisions and potential earnings. Variables that affect only earnings do not need to be included.

2. Variables that would make for appropriate exclusion restrictions in a control function model are not suitable in a matching/imputation model, and vice versa. For example, husband's employment characteristics are often used as exclusion restrictions in modelling married women's employment and wages. Although the assumption that these are excludable in the present context is rejected, it would be perverse to used these in a matching model which make the opposite assumption i.e. that married women in and out of work who have husbands with similar earnings are more like than unlike. See the discussion in chapter 6 (p.52) for a detailed explanation of the difference in the role of the propensity score in matching and control function models.

3. It is clearly necessary to avoid matching on information that excludes the possibility of a plausible, random reason for the difference in actual employment status at a given survey e.g. for women, I have implicitly treated as random *small* differences in the timing of childbirth and in the length of time out of paid work

after having children. Also, occupational and employer characteristics cannot be included since these are missing for non-working individuals.

4. Variables should not have too many missing items. e.g. information on partner's earnings and employment would not have been used on this basis even had it not been rejected on the grounds that it was potentiall excludable from the wage model.

Coming from a larger family, having poorly educated parents, being born to a younger mother, being a younger sibling and having a father in a lower status job have all been found to be markers of childhood disadvantage. There is also evidence that these childhood factors have shaped employment and earnings prospects in adulthood for the three birth cohorts (Kuh and Wadsworth, 1991; Kuh et al., 1997; Schoon et al., 2002; Blanden et al., 2007; Plewis and Kallis, 2008; Flouri and Hawkes, 2008). On this basis, these variables have been included in the probit models used to estimate the propensity score.

In adulthood, the variables included in the probit model are:

- level of highest qualification obtained;
- years spent in full-time work;
- years spent in part-time work;
- region of residence;
- whether any children under 16 are living in the household;
- whether there is more than one child in the household; and
- whether there are children aged under five years old in the household.

For the 1958 and 1970 cohorts, information on the social class status of the first job after leaving full-time education was also included. These variables have all previously been shown to be strong predictors of wages and employment patterns for cohort members (Joshi and Paci, 1998; Blundell et al., 2000; Dolton et al., 2002; Joshi et al., 2007). Variable definitions are given in table 4.10 in chapter 4.

5.2.4 Pooled vs. separate probit regression models

The probit models have been estimated separately for each cohort-age-gender group.[9] A suitable wage 'donor' is always of the same sex, age and cohort. Importantly, no assumption is made about the similarity of potential wages for individuals across cohorts, ages or the two sexes, even if they have similar characteristics.

The probit models discriminate between employees with observed wages, on the one hand, and non-workers, self-employees and employees with missing wages, on the other. In the present analysis, I have used a set of pooled probit models to generate propensity scores. The pooled model would be unsuitable if the pattern of selection into self-employment or the predictors of not reporting wages were qualitatively different to the pattern of selection into paid work. To test this, a set of multinomial logistic regression models were estimated, discriminating between each of the four groups. These showed that the self-employed and missing wage groups were heterogeneous, with very few statistically significant predictors of being self-employed or having a missing wage (vs. being employed with an observed wage). One characteristic that did discriminate self-employees from employees for men was father's social class status. This was also the only characteristic which was a significantly different predictor of self-employment vs. non-employment; self-employed men tended to have been born to fathers in higher social class jobs (most likely self-employees themselves), where unemployed men tended to have been born to fathers in lower social class jobs. Given that the groups were heterogeneous and that the propensity score (and wage level) was most strongly determined by qualifications and employment experience, the pooled model was used.

In a similar vein, although the focus of the analysis in this chapter is not on the differences between women working part-time and full-time, these differences might matter in a practical way for successful matching. If the patterns of differences between full-time employees, part-time employees and non-employed were qualitatively different, a better wage match might be made by using a propensity score estimated from a multinomial model, discriminating between each of the three groups. However, multinomial logistic models distinguishing part-time and full-time work only revealed important differences for women in the 1946 cohort at age 31. At this age, the part-time female workforce was less qualified than the non-working female population, whereas the full-time workforce was more qualified. However, on the basis that the simplest probit model was nested in more complex models for all but this one survey, the simplest models were used.

[9] In total, probit models were estimated for 18 samples.

5.3 Results

5.3.1 Probit models used to estimate the propensity score

Women

Table 5.3 shows the probit regression parameters from the models used to estimate propensity scores for women. These coefficients pick up the relationship between each characteristic and the (cumulative) probability of being out of work. Although self-employees and employees with missing wages are included in the non-employed group, these groups are very small (see tables 5.1 and table 4.3 in chapter 4) and the estimated parameters are not sensitive to their inclusion. The results discussed in this section are based on the final models shown in table 5.3 and also on models including childhood characteristics on their own (not shown) and childhood plus educational characteristics (not shown).

Having a father with a lower status job (defined by occupation) when young and scoring poorly in maths and reading tests at age ten or eleven are consistently positive predictors of being out of work in adult life for the 1958 and 1970 cohorts, although only at age 43 for the 1946 cohort. However, partial correlations between most childhood characteristics and subsequent employment patterns in adulthood are not statistically significant once educational, employment and family characteristics in adulthood are included in the final models.

In all three cohorts, women without formal qualifications are more likely to be out of work in their twenties than women without qualifications. In the two later cohorts, less qualified women are also less likely to be out of work in their thirties. However, the association between education and the probability of being out of work is reversed for the 1946 cohort at ages 26 and 31 once information about children is included in the model.

Social and educational differences in the timing of motherhood complicate patterns of employment for women. The association between having young children (under five years old) in the household and being out of work, holding fixed other characteristics, is large and positive in all of the surveys. The coefficients on some of the social and educational variables become non-significant or reverse their association with employment probabilities once information about motherhood is included in the model. This is most marked for the 1946 cohort samples, in which the association between having a diploma or A-level (versus no formal qualifications) and being out of paid work at age 31 switches from negative or non-significant to positive and significant, once information about children is included in the model.[10] An interpretation of this pattern of correlations is that more educated mothers in the 1946 cohort, who have had children later than average (bearing in mind that the median age was 24), are likely to have had younger children when surveyed at ages 26 and 31 and were consequently more likely to still be out of paid employment at these ages. Unlike in the two later cohorts, nearly all women in the 1946 cohort spent several years out of employment after childbirth.

The association between employment experience and the likelihood of being out of work is negative at all surveys. The association between social class status of a first job and later employment probabilities for the 1958 and 1970 cohorts is more mixed and varies by age and cohort.

Employment rates for mothers are lower in London and the South East than in other parts of Britain, holding fixed other characteristics, for the age 31 survey of the 1946 cohort and the age 30 and 34 surveys of the 1970 cohort. The inclusion of an interaction term showed that the negative association was only for mothers, not other women. This could be related to the higher costs of childcare and transport relative to potential wages, which may reduce or reverse the financial advantages for families associated with mothers returning to paid work. Alternatively, particularly in the 1946 cohort, it could be related to the higher wages of husbands, enabling mothers to stay out of paid work.

[10] Further modelling confirms that it is the presence of young children which changes the signs on the education variables in the model, rather than the inclusion of any of the other characteristics.

Table 5.3: Estimated probit regression parameters from models used to estimate propensity scores, women

	1946 cohort			1958 cohort			1970 cohort		
Age at survey	26	31	43	23	33	42	26	30	34
Father in non-manual job (wt.)	+0.02 (0.10)	+0.13 (0.09)	-0.03 (0.11)	-	-	-	-	-	-
Mother's age at birth (bottom quartile = ref):									
2nd quartile	-0.17 (0.12)	+0.15 (0.11)	+0.02 (0.13)	-0.01 (0.06)	+0.04 (0.05)	-0.06 (0.05)	+0.07 (0.07)	+0.09 (0.06)	+0.11 (0.06)
3rd quartile	-0.14 (0.13)	+0.04 (0.11)	-0.07 (0.13)	-0.07 (0.06)	+0.03 (0.06)	-0.07 (0.06)	+0.04 (0.07)	0.09 (0.06)	+0.11 (0.06)
4th (oldest) quartile	-0.21 (0.14)	+0.09 (0.12)	-0.01 (0.14)	-0.04 (0.07)	+0.03 (0.06)	-0.01 (0.06)	+0.07 (0.08)	+0.03 (0.07)	+0.12 (0.07)
(variable missing)	-0.24 (0.20)	-0.01 (0.20)	0.00 (0.21)	-0.21 (0.10)	-0.02 (0.09)	-0.21 (0.09)	+0.42 (0.62)	-0.56 (0.07)	+0.02 (0.42)
Father's social class (V & VI = ref):									
I	+0.08 (0.23)	-0.10 (0.21)	-0.01 (0.23)	+0.24 (0.10)	-0.05 (0.10)	+0.17 (0.10)	+0.19 (0.13)	+0.12 (0.11)	+0.13 (0.11)
II	+0.03 (0.14)	+0.12 (0.13)	+0.11 (0.15)	+0.06 (0.08)	+0.04 (0.10)	+0.11 (0.07)	+0.13 (0.09)	+0.07 (0.08)	+0.01 (0.08)
III	+0.15 (0.14)	-0.23 (0.13)	+0.07 (0.15)	-0.03 (0.08)	+0.03 (0.07)	-0.02 (0.08)	0.00 (0.11)	+0.12 (0.10)	+0.07 (0.09)
IV	-0.04 (0.12)	-0.12 (0.10)	+0.14 (0.12)	+0.02 (0.06)	-0.09 (0.05)	-0.03 (0.05)	+0.03 (0.08)	+0.11 (0.07)	0.00 (0.06)
(variable missing)	-0.04 (0.19)	-0.41 (0.18)	+0.14 (0.21)	+0.12 (0.07)	-0.06 (0.06)	+0.02 (0.07)	+0.11 (0.19)	-0.08 (0.18)	-0.02 (0.17)
Mother's schooling (min = ref):									
Left at 17	-0.12 (0.21)	-0.12 (0.19)	-0.01 (0.23)	-0.05 (0.11)	+0.05 (0.10)	-0.03 (0.11)	-0.04 (0.10)	-0.17 (0.09)	+0.05 (0.08)
Left at 18	+0.09 (0.26)	+0.24 (0.25)	+0.01 (0.26)	+0.10 (0.10)	-0.01 (0.10)	+0.08 (0.10)	0.00 (0.11)	-0.02 (0.10)	-0.10 (0.09)
(variable missing)	+0.82 (0.31)	+0.74 (0.19)	+0.02 (0.33)	+0.06 (0.16)	0.00 (0.15)	-0.23 (0.15)	-0.42 (0.62)	+0.54 (0.37)	+0.16 (0.42)
Father's schooling (min = ref):									
Left at 17	+0.22 (0.20)	0.00 (0.19)	+0.27 (0.21)	-0.15 (0.12)	-0.05 (0.11)	+0.05 (0.12)	+0.03 (0.11)	-0.11 (0.10)	+0.04 (0.09)
Left at 18	-0.16 (0.22)	-0.08 (0.20)	+0.14 (0.21)	-0.05 (0.09)	+0.04 (0.09)	+0.04 (0.09)	+0.05 (0.10)	+0.10 (0.09)	+0.04 (0.09)
(variable missing)	-0.64 (0.30)	-0.70 (0.31)	+0.26 (0.32)	-0.09 (0.12)	-0.16 (0.10)	+0.11 (0.12)	+0.20 (0.19)	+0.26 (0.17)	-0.03 (0.17)
Siblings, at age 16 (4+ = ref):									
Only child	+0.03 (0.19)	+0.01 (0.17)	+0.07 (0.20)	0.00 (0.11)	+0.29 (0.15)	-0.04 (0.10)	-0.25 (0.16)	-0.14 (0.13)	+0.12 (0.13)
One sibling	+0.30 (0.16)	+0.07 (0.14)	+0.12 (0.16)	-0.03 (0.08)	+0.19 (0.11)	+0.09 (0.08)	-0.22 (0.14)	-0.14 (0.11)	+0.15 (0.11)
Two or three siblings	+0.26 (0.13)	+0.05 (0.12)	+0.09 (0.14)	+0.04 (0.07)	+0.12 (0.07)	+0.07 (0.06)	-0.22 (0.14)	-0.23 (0.10)	+0.10 (0.10)
(variable missing)	-0.10 (0.25)	+0.29 (0.23)	-0.31 (0.27)	0.00 (0.16)	+0.29 (0.15)	+0.13 (0.15)	-0.27 (0.14)	-0.12 (0.11)	+0.12 (0.11)
Older siblings (2+ = ref):									
No older sibling	-0.11 (0.14)	-0.07 (0.13)	-0.05 (0.15)	-0.03 (0.08)	-0.05 (0.07)	-0.11 (0.07)	+0.01 (0.09)	-0.02 (0.08)	-0.03 (0.08)
One older sibling	-0.04 (0.13)	-0.07 (0.11)	+0.10 (0.13)	-0.06 (0.07)	0.00 (0.06)	-0.09 (0.07)	+0.07 (0.09)	+0.03 (0.07)	-0.06 (0.07)

Table 5.3: Estimated probit regression parameters from models used to estimate propensity scores, women - continued

	1946 cohort			1958 cohort			1970 cohort		
Age at survey	26	31	43	23	33	42	26	30	34
Maths score at age 11	-0.11 (0.07)	-0.08 (0.06)	-0.02 (0.07)	-0.08 (0.03)	-0.07 (0.03)	-0.02 (0.03)	-0.10 (0.04)	-0.06 (0.03)	-0.03 (0.03)
Reading score at age 11	+0.10 (0.07)	+0.12 (0.06)	+0.02 (0.07)	+0.10 (0.04)	+0.03 (0.03)	+0.06 (0.03)	+0.01 (0.04)	+0.03 (0.03)	-0.02 (0.03)
(missing maths score)	+0.29 (0.17)	+0.06 (0.16)	+0.28 (0.18)	-0.06 (0.06)	-0.07 (0.05)	0.00 (0.05)	+0.03 (0.06)	+0.02 (0.05)	+0.04 (0.05)
Highest qualification (no quals = ref):									
O-level or equivalent	+0.06 (0.12)	0.00 (0.10)	-0.29 (0.12)	-0.17 (0.06)	-0.03 (0.05)	+0.04 (0.05)	-0.18 (0.06)	-0.08 (0.06)	-0.11 (0.06)
A-level or equivalent	-0.16 (0.15)	+0.20 (0.13)	-0.17 (0.15)	-0.41 (0.08)	-0.05 (0.07)	+0.02 (0.07)	-0.12 (0.09)	+0.02 (0.07)	-0.12 (0.07)
Diploma	+0.07 (0.17)	+0.35 (0.16)	-0.52 (0.18)	-0.47 (0.09)	-0.21 (0.07)	-0.11 (0.07)	-0.41 (0.10)	-0.27 (0.07)	-0.38 (0.07)
Degree or higher	+0.19 (0.22)	+0.11 (0.21)	-0.06 (0.17)	-0.72 (0.11)	-0.56 (0.09)	-0.29 (0.08)	-0.15 (0.10)	-0.75 (0.08)	-0.68 (0.07)
Social class of first job (V = ref):									
I	-	-	-	+0.01 (0.16)	+0.57 (0.17)	+0.40 (0.16)	-0.59 (0.18)	-0.44 (0.15)	-0.12 (0.13)
II	-	-	-	-0.21 (0.08)	-0.04 (0.07)	-0.08 (0.08)	-0.34 (0.08)	-0.26 (0.07)	-0.15 (0.07)
III	-	-	-	+0.03 (0.06)	+0.01 (0.05)	+0.01 (0.05)	-0.20 (0.07)	-0.22 (0.05)	-0.12 (0.05)
IV	-	-	-	+0.23 (0.08)	+0.12 (0.20)	-0.05 (0.08)	+0.10 (0.09)	+0.11 (0.07)	-0.18 (0.07)
VI	-	-	-	+0.20 (0.23)	-0.12 (0.20)	-0.15 (0.21)	-0.10 (0.18)	-0.40 (0.15)	-0.26 (0.15)
(missing)	-	-	-	+1.28 (0.22)	+0.05 (0.11)	-0.33 (0.08)	+0.09 (0.12)	+0.46 (0.14)	-0.01 (0.09)
Years in full-time emp	-0.14 (0.01)	-0.18 (0.02)	-0.10 (0.01)	-0.23 (0.01)	-0.10 (0.01)	-0.09 (0.00)	-0.05 (0.01)	-0.15 (0.01)	-0.10 (0.01)
Years in part-time emp	-0.20 (0.01)	-0.16 (0.03)	-0.14 (0.01)	-0.57 (0.04)	-0.16 (0.01)	-0.11 (0.00)	-0.09 (0.02)	-0.21 (0.01)	-0.12 (0.01)
Children in hhld	+0.26 (0.17)	+0.73 (0.11)	-0.09 (0.12)	+0.42 (0.14)	+0.20 (0.06)	-0.09 (0.06)	+0.75 (0.11)	+0.26 (0.07)	+0.03 (0.06)
Children under five	+0.92 (0.12)	d	+1.10 (0.21)	+1.16 (0.15)	+0.64 (0.04)	+0.67 (0.06)	+0.14 (0.11)	+0.62 (0.06)	+0.48 (0.05)
More than one child in hhld	-0.20 (0.12)	d	-0.09 (0.11)	-0.02 (0.09)	-0.01 (0.05)	-0.07 (0.05)	+0.33 (0.08)	+0.06 (0.05)	+0.12 (0.05)
Living in London/SE	+0.02 (0.09)	+0.30 (0.08)	-0.06 (0.09)	-0.05 (0.07)	+0.10 (0.04)	+0.13 (0.04)	-	+0.15 (0.04)	+0.14 (0.04)
Constant term	+0.68 (0.21)	+0.14 (0.19)	+0.81 (0.24)	+0.81 (0.13)	+0.64 (0.11)	+1.07 (0.11)	-0.15 (0.16)	+0.83 (0.14)	+0.88 (0.14)
Pseudo R-squared	0.47	0.16	0.16	0.34	0.16	0.15	0.15	0.26	0.15
Sample size	1,710	1,431	1,167	5,721	5,682	5,764	3,836	5,735	5,014

The coefficients shown are estimated probit regression parameters from nine probit models, with standard errors in brackets. These model the probability of being one of non-employed, self-employed or having a missing wage vs. being employed with an observed wage.
- indicates that variables are missing. d indicates that a variable was dropped from the model owing to collinearity.

Men's employment

Table 5.4 shows the estimated probit regression parameters from models used to predict propensity scores for men. The results are from pooled probit models, including non-employed, self-employed and employed with missing wages all in the same group. A significant fraction of men were self-employed in their thirties and forties in each of the three cohorts (between 10 and 21 per cent). Further modelling (not presented in tables here) suggested that self-employed men are a heterogeneous group. For the most part, they appear to be less highly-qualified than male employees, but more likely to have had fathers in social class II, which includes self-employees.

For men, the markers of childhood disadvantage and lower maths and reading ability have consistently positive associations with being in work, which includes self-employment (not shown) in each survey. These associations are weakened but not reversed in the final pooled model. Table 5.4 shows the negative associations between higher educational attainment and non-employment probabilities, except for the early surveys of the 1946 cohort (at which point though, 95 per cent of men in the cohort were employed). There is also a negative association between employment experience and the probability of being out of work. Lower social class status of a first job is a predictor of unemployment later on.

Table 5.4: Estimated probit regression parameters from models used to estimate propensity scores, men

	1946 cohort			1958 cohort			1970 cohort		
	26	31	43	23	33	42	26	30	34
Father in non-manual job (wt.)	+0.12 (0.09)	0.00 (0.09)	-0.07 (0.10)	-	-	-	-	-	-
Mother's age at birth (bottom quartile = ref):									
Second quartile	+0.01 (0.11)	0.00 (0.11)	-0.08 (0.12)	-0.06 (0.05)	-0.10 (0.05)	-0.04 (0.05)	+0.07 (0.07)	-0.03 (0.06)	+0.06 (0.06)
Third quartile	-0.14 (0.12)	+0.02 (0.11)	-0.09 (0.12)	0.00 (0.06)	-0.10 (0.05)	-0.16 (0.06)	+0.01 (0.08)	-0.01 (0.07)	+0.03 (0.07)
Oldest quartile	+0.03 (0.12)	-0.06 (0.12)	-0.05 (0.13)	+0.05 (0.06)	-0.04 (0.06)	-0.05 (0.06)	+0.13 (0.08)	-0.02 (0.07)	-0.06 (0.07)
(missing)	-0.02 (0.18)	+0.23 (0.18)	+0.33 (0.19)	0.00 (0.09)	-0.12 (0.09)	-0.05 (0.09)	0.00 (0.38)	-0.14 (0.31)	+0.33 (0.32)
Father's social class (V & VI = ref):									
I	-0.07 (0.20)	-0.06 (0.19)	0.00 (0.20)	+0.04 (0.10)	0.14 (0.10)	+0.20 (0.10)	-0.24 (0.13)	+0.03 (0.11)	+0.22 (0.11)
II	+0.13 (0.12)	+0.08 (0.12)	+0.05 (0.13)	+0.16 (0.07)	+0.26 (0.07)	+0.24 (0.07)	-0.08 (0.09)	+0.11 (0.08)	+0.22 (0.08)
III	+0.04 (0.12)	-0.06 (0.12)	-0.13 (0.14)	+0.05 (0.08)	-0.03 (0.08)	-0.05 (0.08)	-0.22 (0.11)	-0.20 (0.10)	-0.04 (0.10)
IV	+0.23 (0.10)	-0.04 (0.10)	-0.05 (0.11)	+0.05 (0.05)	+0.06 (0.05)	+0.09 (0.05)	-.13 (0.08)	-0.12 (0.06)	+0.02 (0.07)
(missing)	-0.12 (0.17)	-0.03 (0.17)	+0.06 (0.18)	+0.10 (0.07)	+0.15 (0.06)	+0.02 (0.07)	-0.28 (0.20)	-0.35 (0.16)	-0.03 (0.17)
Mother's schooling (min = ref):									
Left at 17	-0.13 (0.19)	+0.24 (0.18)	-0.38 (0.22)	0.00 (0.11)	-0.12 (0.11)	+0.14 (0.11)	+0.04 (0.10)	+0.09 (0.30)	0.00 (0.09)
Left at 18	-0.20 (0.25)	+0.12 (0.24)	+0.32 (0.25)	-0.02 (0.10)	+0.15 (0.10)	+0.13 (0.11)	+0.05 (0.11)	+0.02 (0.09)	-0.08 (0.10)
(missing)	+0.05 (0.25)	-0.10 (0.27)	-0.06 (0.30)	+0.11 (0.15)	+0.15 (0.14)	+0.12 (0.15)	+0.08 (0.37)	+0.09 (0.30)	-0.21 (0.32)
Father's schooling (min = ref):									
Left at 17	-0.09 (0.17)	-0.26 (0.17)	+0.28 (0.18)	+0.09 (0.11)	+0.15 (0.11)	+0.14 (0.12)	+0.13 (0.11)	-0.13 (0.10)	-0.08 (0.10)
Left at 18	+0.10 (0.20)	+0.11 (0.18)	+0.11 (0.20)	+0.07 (0.09)	-0.09 (0.09)	-0.05 (0.09)	+0.13 (0.10)	-0.25 (0.10)	-0.06 (0.09)
(missing)	+0.22 (0.17)	-0.10 (0.25)	-0.35 (0.28)	+0.08 (0.12)	-0.06 (0.12)	+0.09 (0.12)	+0.20 (0.20)	+0.27 (0.16)	-0.02 (0.17)
Siblings, age 16 (4+ = ref):									
Only child	-0.19 (0.17)	-0.17 (0.17)	+0.11 (0.20)	+0.07 (0.11)	-0.02 (0.11)	+0.10 (0.11)	-0.33 (0.17)	+0.12 (0.14)	+0.22 (0.14)
One sibling	-0.07 (0.14)	-0.16 (0.14)	+0.26 (0.15)	+0.12 (0.08)	0.00 (0.08)	+0.10 (0.08)	-0.32 (0.15)	0.00 (0.12)	+0.10 (0.12)
Two or three siblings	-0.13 (0.11)	+0.05 (0.12)	+0.13 (0.13)	+0.12 (0.06)	+0.04 (0.06)	+0.03 (0.06)	-0.27 (0.14)	+0.04 (0.10)	+0.16 (0.11)
(missing)	+0.25 (0.22)	+0.30 (0.22)	+0.36 (0.24)	-0.05 (0.14)	-0.05 (0.13)	-0.11 (0.14)	-0.24 (0.15)	+0.06 (0.11)	+0.09 (0.12)
Older siblings (2+ = ref):									
No older sibling	0.00 (0.13)	+0.08 (0.13)	-0.06 (0.14)	-0.02 (0.07)	-0.06 (0.07)	-0.11 (0.07)	+0.09 (0.09)	-0.10 (0.08)	-0.17 (0.08)
One older sibling	+0.07 (0.11)	+0.12 (0.11)	+0.14 (0.12)	+0.04 (0.07)	+0.02 (0.07)	+0.05 (0.07)	+0.14 (0.09)	-0.05 (0.07)	-0.05 (0.07)

Table 5.4: Estimated probit regression parameters from models used to estimate propensity scores, men - continued

	1946 cohort			1958 cohort			1970 cohort		
Age at survey	26	31	43	23	33	42	26	30	34
Maths score at age 11	-0.02 (0.06)	-0.14 (0.05)	+0.01 (0.06)	+0.07 (0.03)	-0.04 (0.03)	-0.03 (0.03)	-0.10 (0.04)	-0.02 (0.03)	+0.02 (0.03)
Reading score at age 11	+0.02 (0.05)	+0.05 (0.05)	+0.01 (0.06)	-0.09 (0.03)	-0.05 (0.03)	-0.05 (0.03)	+0.02 (0.04)	-0.01 (0.03)	-0.01 (0.03)
(missing score)	-0.31 (0.17)	-0.35 (0.17)	-0.04 (0.17)	-0.07 (0.05)	-0.02 (0.05)	+0.02 (0.05)	+0.02 (0.06)	+0.11 (0.05)	+0.05 (0.05)
Highest qualification (no quals = ref):									
O-level or equivalent	+0.01 (0.12)	-0.04 (0.12)	-0.05 (0.13)	-0.15 (0.05)	+0.01 (0.05)	+0.10 (0.06)	-0.14 (0.07)	-0.07 (0.06)	+0.08 (0.06)
A-level or equivalent	-0.06 (0.12)	0.00 (0.12)	-0.05 (0.12)	-0.29 (0.05)	-0.12 (0.06)	+0.09 (0.06)	-0.29 (0.09)	-0.18 (0.06)	-0.01 (0.06)
Diploma	-0.11 (0.13)	-0.32 (0.13)	-0.27 (0.14)	-0.34 (0.08)	0.31 (0.07)	-0.14 (0.07)	-0.34 (0.10)	-0.44 (0.08)	-0.18 (0.07)
Degree or higher	-0.29 (0.15)	+0.01 (0.14)	-0.26 (0.14)	-0.81 (0.10)	-0.64 (0.08)	-0.48 (0.08)	-0.26 (0.09)	-0.84 (0.08)	-0.48 (0.08)
Social class of first job (V = ref):									
I	-	-	-	-0.29 (0.10)	+0.05 (0.11)	+0.15 (0.11)	-0.28 (0.12)	-0.20 (0.12)	-0.16 (0.11)
II	-	-	-	-0.14 (0.08)	-0.26 (0.08)	-0.17 (0.08)	-0.30 (0.08)	-0.20 (0.07)	-0.10 (0.07)
III	-	-	-	-0.17 (0.06)	-0.24 (0.06)	0.00 (0.06)	-0.17 (0.08)	-0.22 (0.07)	-0.15 (0.07)
IV	-	-	-	+0.09 (0.06)	-0.02 (0.05)	-0.11 (0.08)	+0.08 (0.07)	+0.11 (0.06)	+0.22 (0.06)
VI	-	-	-	+0.02 (0.08)	-0.06 (0.08)	-0.33 (0.08)	+0.48 (0.12)	+0.13 (0.09)	+0.26 (0.10)
(missing)	-	-	-	+0.46 (0.10)	-0.15 (0.09)	-0.33 (0.08)	+0.15 (0.10)	+0.17 (0.10)	+0.16 (0.08)
Years in full-time emp	-0.18 (0.26)	-0.18 (0.05)	-0.18 (0.02)	-0.25 (0.01)	-0.10 (0.01)	-0.07 (0.00)	-0.02 (0.01)	-0.12 (0.01)	+0.08 (0.01)
Years in part-time emp	-0.03 (0.05)	+0.08 (0.13)	-0.18 (0.05)	-0.17 (0.07)	-0.06 (0.03)	-0.03 (0.01)	-0.01 (0.04)	-0.07 (0.02)	0.00 (0.02)
Children in hhld	-0.18 (0.02)	d	+0.08 (0.10)	+0.16 (0.20)	+0.01 (0.06)	-0.18 (0.06)	+0.14 (0.15)	-0.02 (0.09)	-0.03 (0.07)
Children under five	+0.30 (0.25)	d	+0.04 (0.14)	-0.09 (0.20)	-0.03 (0.05)	+0.08 (0.05)	-0.31 (0.15)	-0.01 (0.08)	-0.08 (0.06)
More than one child in hhld	+0.13 (0.11)	d	-0.05 (0.11)	-0.07 (0.10)	+0.04 (0.05)	+0.08 (0.05)	+0.16 (0.12)	+0.13 (0.07)	+0.12 (0.06)
Living in London/SE	+0.07 (0.08)	+0.09 (0.08)	-0.03 (0.09)	-0.01 (0.06)	+0.07 (0.04)	+0.04 (0.04)	-	+0.08 (0.04)	+0.07 (0.04)
Constant term	+1.08 (0.23)	+0.46 (0.30)	+2.52 (0.44)	+0.93 (0.12)	+1.04 (0.12)	+1.01 (0.12)	+0.02 (0.17)	+0.86 (0.15)	+0.45 (0.16)
Pseudo R-squared	0.11	0.04	0.08	0.10	0.07	0.08	0.04	0.10	0.07
Sample size	1,768	1,467	1,283	5,535	5,441	5,585	3,367	5,360	4,588

The coefficients shown are estimated probit regression parameters from nine separate probit models, with standard errors given in brackets. These model the probability of being either non-employed, self-employed or having a missing wage versus being employed, with an observed wage.
- indicates that variables are missing. d indicates that a variable was dropped from the model owing to collinearity.

5.3.2 Multivariate analysis of employment and wages

In addition to the estimated probit regression models, used directly in the imputation model to generate propensity scores, a set of log-linear wage models, containing the same selected variables, were estimated for the employed samples who had an observed wage. These wage models formed the basis for an alternative set of predicted potential wages for non-employees, the results of which are not presented in this chapter. They also though, provided a useful set of results in themselves for the purpose of understanding educational and social differences in patterns of employment and wages for women and men in each cohort.

Summarising the results from the set of probit and wage regression models, these showed that higher qualifications, higher childhood maths ability, higher status upon entering the job market and continuity of employment are all consistently and systematically associated higher employment probabilities and with higher wages when employed. This is the straightforward pattern we would expect if we assumed that higher potential wages incentivise being in work. It is also what we would see if employers offering lower wages also offered less security of employment.

Women's employment patterns are complicated by the fact that the timing of childbirth differs by social and educational status. More educated women are likely to have children later in life. As a consequence, non-employed women become a more diverse group with age. For the 1946 cohort, nearly all new mothers spent several years out of employment but there are social and educational differences in the timing of childbirth, spread out over a relatively short period (early-twenties to early-thirties). As a consequence, at age thirty-one, by which age most women in the cohort had become mothers (see fig. 5.2), there are few systematic differences between the group in and out of employment. In contrast, in the 1958 and 1970 cohorts, systematic educational differences persist at older ages, although they also become weaker with age. This is because the composition effects of the delay in childbirth amongst more qualified women are partly offset by the more rapid return to work amongst this group after childbirth.

In the 1958 and 1970 cohorts, the results from the employment and wage models for women, taken together, suggest positive selection into later motherhood and/or positive selection into work when children are young amongst older mothers. The indication of positive selection comes from the fact that the partial correlation between having small children and being in work is negative, but the partial correlation between having small children and wages is positive for mothers in work in their thirties and forties. The selection effect (i.e. the opposite signs on the coefficients on having small children in the wage and employment models) appears to be stronger in London and the South East. It is unclear whether positive selection into later motherhood or positive selection into employment amongst older mothers is behind this pattern. The issue of unobserved selectivity bias is explored further in the section following a more general description of the goodness of the imputation strategy.

5.3.3 Details of imputations

Some important practical questions about the imputations are addressed briefly here, including:

1. How many imputations have been made in each sample?
2. How far do the distributions of propensity scores for employees and non-employees overlap in each sample?
3. How well does matching on the propensity score balance the distributions of variables across the employed and non-employed groups?

The aim of the imputation exercise was to impute potential wages for as many non-employed individuals as possible, since the proportion, as well as the potential wages of non-employed individuals, affect the overall estimates of average potential pay.[11] For this reason, no stringent limits were placed on how numerically close the propensity scores had to be before the imputation was accepted. A common support restriction was used, which meant not imputing potential wages for non-employed individuals who had a propensity score lower than the *lowest* propensity score estimated for any employed individual in the same sample. Tables 5.5 and 5.6 show the numbers of individuals in each sample for whom imputations were made and not made. Very few imputations failed because of the common support restriction. In appendix 4.3, figures 5.11 and 5.12 show the overlap in the distributions of propensity scores for those in and out of employment at each sample. They also show that the observed differences between those in and out of work are greatest amongst women at younger ages.

Another issue is how well matching on the propensity score manages to balance the distributions of actual variables across the groups in and out of work. In appendix 4.3, tables summarise differences in specific variables between the group with imputed potential wages (including those out of work, self-employees and employees with missing

[11] This is different from the application of propensity score matching to research questions in which differences in outcomes between smaller sub-samples of very similar matched individuals are of interest.

Table 5.5: Numbers of observed and imputed cases for women in each sample

	1946 cohort			*1958 cohort*			*1970 cohort*		
Age at survey	*26*	*31*	*43*	*23*	*33*	*42*	*26*	*30*	*34*
Observed wage	739	630	1,129	3,400	3,078	3,999	3,237	3,894	2,986
Imputed potential wage, of which	969	878	299	2,518	2,655	1,769	1,202	1,804	2,031
Not employed	845	684	183	1,947	1,816	1,207	802	1,459	1,280
Self-employed	50	70	88	87	385	426	152	261	300
Employed, missing wage	74	124	25	449	439	135	248	83	451
No imputation, of which	152	139	155	338	52	9	396	68	8
No support	41	3	4	86	2	3	11	46	1
Missing covariates	101	136	151	252	50	6	385	22	7
Total*	1,860	1,647	1,583	6,256	5,785	5,777	4,835	5,766	5,025

Table 5.6: Numbers of observed and imputed cases for men in each sample

	1946 cohort			*1958 cohort*			*1970 cohort*		
Age at survey	*26*	*31*	*43*	*23*	*33*	*42*	*26*	*30*	*34*
Observed wage	1,463	1,184	1,105	4,084	3,697	3,959	2,779	4,167	3,270
Imputed potential wage, of which	356	408	363	1,752	1,816	1,622	972	1,147	1,319
Not employed	74	60	51	788	503	507	385	451	319
Self-employed	163	189	285	304	865	992	333	605	658
Employed, missing wage	119	159	27	619	436	121	254	89	342
No imputation, of which	78	69	124	356	69	24	312	383	20
No support	18	3	16	69	12	9	4	75	13
Missing covariates	60	66	108	344	57	15	308	47	7
Total*	1,897	1,661	1,592	6,249	5,582	5,605	4,063	5,436	4,609

*This total includes nearly all of those who responded to the survey, excluding only those individuals who responded but did not report their current main economic activity.

wages) and the group of employees who form the group of matched wage donors. These show non-significant differences for all the male samples, but significant variables for women across selected variables. Appendix 4.3 also gives more information on the effectiveness of propensity scores generated by a pooled probit model (discriminating between employees with observed wages, on the one hand, and all other groups without observed wages, on the other) in discriminating between employees and the different missing-wage groups, as opposed to a set of separate probit models. The conclusion is that the pooled model is adequate.

5.3.4 Signs of unobserved selectivity bias

Three data exercises were used to investigate possible unobserved selectivity biases. First, the estimated coefficients on variables in the probit models (Table 5.3) were compared to a set of coefficients on the same variables in log-linear wage models estimated by least squares (not shown). For the 1958 and 1970 cohorts, gthere were signs of positive selection for women in their thirties and forties. Having young children was a positive predictor both of being out of work at these ages and of having higher wages when in work. This result is suggestive of either: positive selection into motherhood at older ages, which would not affect our wage imputations; or, positive selection into work amongst older mothers, which would result in an upward bias in imputed potential wages for these samples.

A second exercise was to compare the means of each characteristic used in the probit models across the matched samples. We found that the means were generally not significantly different across the matched samples, with some important exceptions. For the 1946 cohort at age 26 and for the 1958 cohort at age 23, working women as a whole group had higher levels of education and higher average maths scores than non-working women. In contrast, the matched working samples had fewer qualifications and significantly lower scores than the matched non-working samples. This pattern suggests negative selection into work amongst mothers at these ages, not adequately accounted for by the matching exercise. For the 1946 cohort, within the group of mothers with young children, it was less educated mothers 21 who had given birth slightly earlier and were more likely to have returned to work by the age of 26. Imputed wage offers for these non-working groups may consequently be biased downwards. The size of this downward bias is likely to be small and our estimated trends appear to be robust to alternative specifications, e.g. imputing the predicted wage from a log-linear wage model.

A third investigation was carried out for women in the 1958 cohort only, using additional information collected about their weekly wage in their first job after leaving full-time education, requested at the age 23 survey. For the 281 women who provided this information, there was a difference in mean log first wage across the unmatched working and non-working samples, which was marginally significant (at the 10% level), supporting the view that observed selection biases show up slightly in the first wage.

In contrast, there was no difference in mean log first wage across the matched working and non-working samples, suggesting that the matching successfully removed any fixed, unobserved biases that appear in the first wage. Positive selectivity biases associated with job search intensity or with having a better potential employer would not show up in the tests carried out.

Gronau's (1974) observation that similar individuals face a range of wage offers, and that the better wage offers are more likely to be accepted, would further imply that the imputed wage offers in our analysis overstate the actual wage offers faced by non-working individuals. Without exclusion restrictions, it is not possible to quantify the likely size of such biases, although the evidence in the literature (discussed in chapter 3) suggests that these are likely to be small relative to those captured by differences in education and work experience.

5.3.5 Description of potential wage distributions

Tables 5.7 and 5.8 show median values for employees with observed wages at each survey and for the group with imputed potential wages, plus a breakdown for the different imputation groups. For women, in the majority of surveys, those who are out of work have much lower median potential wages than those in work and it is the imputed values for non-employees which drag down the median for the whole imputed group. Self-employees and employees with missing wages do not have systematically lower wages in the majority of surveys and, at any rate, comprise only a small fraction of imputed cases (see table 5.5).

Table 5.7: Median observed wages and imputed potential wages for women, by survey (£, 2000 prices)

	1946 cohort			*1958 cohort*			*1970 cohort*		
Age at survey	*26*	*31*	*43*	*23*	*33*	*42*	*26*	*30*	*34*
Observed wage	4.06	3.95	5.07	4.76	6.26	6.51	6.33	7.10	7.83
Imputed potential wage, of which	3.33*	3.74*	4.69	3.92*	5.00*	5.55*	5.71*	5.30*	6.32*
Not employed	3.33	3.71	4.35	3.84	4.73	5.34	5.43	5.13	5.38
Self-employed	4.23	3.63	5.14	4.37	5.74	6.38	6.46	6.66	7.47
Employed, missing wage	3.84	3.94	5.87	4.85	6.14	5.54	6.44	6.39	7.66

*Difference from median observed wage is statistically significant at the 5% level.

For men, the imputed potential wages of those not in work are also systematically lower than the wages of men in work at most of the surveys. However, in the majority of the surveys, these comprise a small number of the imputed cases, relative to the number of imputations for male self-employees and missing wage cases (see table 5.6). As a consequence, the median potential wage for the whole imputed group of men is systematically lower than the median wage for employed men (with observed wages) in around half of the surveys (table 5.8).

Table 5.8: Median observed wages and imputed potential wages for men, by survey (£, 2000 prices)

	1946 cohort			*1958 cohort*			*1970 cohort*		
Age at survey	*26*	*31*	*43*	*23*	*33*	*42*	*26*	*30*	*34*
Observed wage	5.94	6.29	8.50	5.69	8.96	9.70	7.06	8.21	9.72
Imputed potential wage, of which	5.40*	6.47	8.53	5.61	8.11*	8.39*	6.90	7.24*	8.69*
Not employed	4.79	6.01	8.19	5.50	7.10	7.02	6.96	6.76	7.05
Self-employed	5.43	6.43	9.30	5.77	8.31	9.10	6.61	7.72	9.57
Employed, missing wage	5.42	6.82	7.73	5.66	8.52	8.94	7.02	7.17	9.04

*Difference from median observed wage is statistically significant at the 5% level.

Figure 5.5 compares the median observed wage for women employees to the estimated median potential wage for non-employed women and the median for the full sample, including employees, non-employees and self-employees. Medians are not shown separately for self-employees. Figure 5.9 in appendix 5.1 shows wage distributions for employees and potential wage distributions for the whole samples.

For women in their twenties and thirties, the size of the difference between the median imputed potential wage for non-employees and median observed wage for employees has increased across the three cohorts (Figure 5.5). The increase in the strength of the selection effect may be owing to the widening of the wage distribution over this period, as well as to increasing differences in the social and educational characteristics of employed and non-employed women. However, the aggregate effect of selectivity bias has reduced across the cohorts, as the fraction of women out of work has decreased. This effect shows up as a decreasing gap between the median observed wage for the employed population and the median potential wage for the whole population.

The error bars (representing the 95% confidence intervals) in figure 5.5 show that the medians calculated from the full distributions are systematically lower than the median observed wage for the 1946 cohort at age 26, for the 1958 cohort at ages 23 and 33 and for the 1970 cohort at ages 30 and 34. These ages cover the average childbearing years for women in each of the cohorts, and at least a fifth of women in each of these surveys describe themselves as being full-time housewives or carers (see table 5.1.)

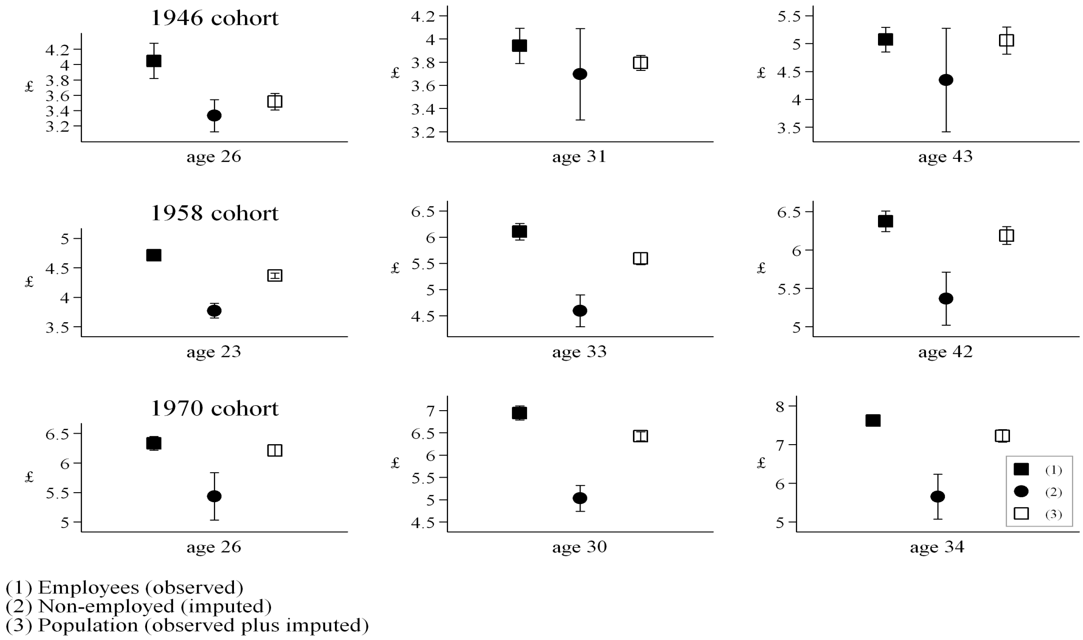

Figure 5.5: Medians of (1) observed wages for employees (2) potential wages for non-employees (3) observed wages and imputed potential wages (employees, self-employees and non-employees), women

Only a small fraction of men were not employed for some period, although the potential wages of the non-employed minority are significantly lower than those of employees for men in the 1958 and 1970 cohorts in their thirties and forties (Figure 5.6). Owing to the small fraction out of work, the median potential wage for the whole male population is not significantly different from the median observed wage for employees at any of the surveys.

5.3.6 Estimates of women's and men's relative pay opportunities

Figure 5.7 compares estimates of the ratio of women's to men's median actual hourly pay at each survey with estimates of their relative median pay opportunity, including observed and imputed values. The error bars on the points (the 95% confidence inter-

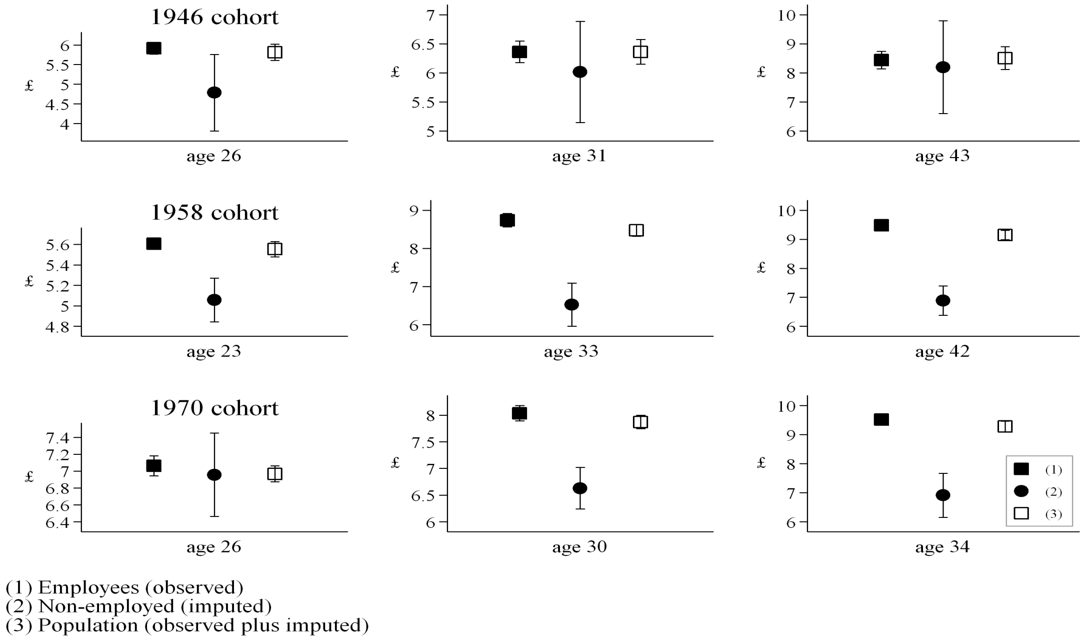

Figure 5.6: Medians of (1) observed wages for employees (2) potential wages for non-employees (3) observed wages and imputed potential wages (employees, self-employees and non-employees), men

vals) are non-overlapping for the 1946 cohort at age 26, for the 1958 cohort at age 23 and, marginally, for the 1970 cohort at age 30.[12]

Taking the trends for women and men together, women's relative wage opportunities have improved across the three cohorts, moving in the same direction as observed wage trends (figure 5.8). However, the increase in younger women's wage opportunities is greater than the observed increase in wages for this group. In particular, the ratio of women's to men's median potential wages is substantially and significantly lower than the observed wage ratio for the 1946 cohort at age 26 (figure 5.7), when only half of women were in work.

There is also weak evidence in the 1970 cohort that women's selective withdrawal and re-entry into the workforce around childbearing years conceals some of the decline in women's labour market position after childbirth. Between the ages of 26 and 30, around a fifth of women in the cohort had a first baby (Figure 5.2). The proportion of women in work fell from around 78% to around 74% (Figure 5.1) and the estimated potential

[12]The error bar on the *difference* in the ratios includes zero for the 1970 cohort sample.

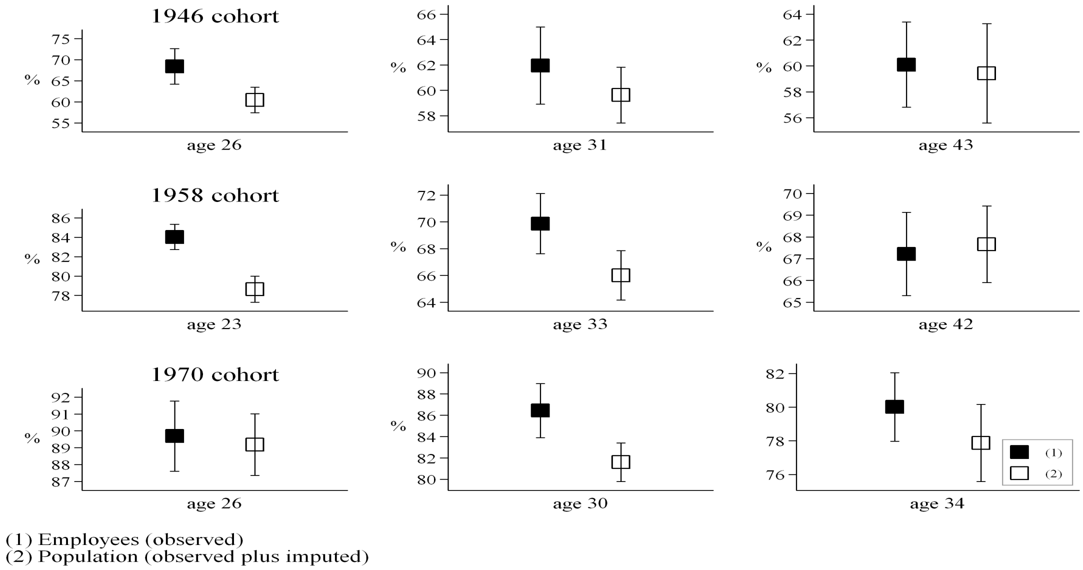

Figure 5.7: Female to male ratios for observed median wages and for observed-plus-imputed median potential wages, by survey

wage gap between employed and non-employed women increased (Figure 5.5). Median hourly wages for working women decreased from 89.7% of mens to 86.5% of men's between these ages. Median potential wages for all women decreased more though, from 89.2% to 81.7% (figure 5.8).

5.4 Discussion

Women's position in the labour market has changed dramatically across the three birth cohorts, with a simultaneous increase in their rates of employment and in their rates of pay, relative to men's. For example, comparing the cohorts when in their early thirties: for the 1946 cohort, women's median hourly wage was around 60 per cent of men's; for the 1958 cohort, the ratio had increased to 70 per cent; and for the 1970 cohort, the ratio had increased again to 80 per cent. Across the same surveys, women's rates of employment increased from just over half for the 1946 cohort, to three-quarters for the 1970 cohort. In contrast, men's employment rates decreased slightly, though they remained high (over 90% for men in their thirties). The cross-cohort change is even more marked when comparing the cohorts in their twenties, but less marked when comparing the 1946 and 1958 cohorts in their forties.

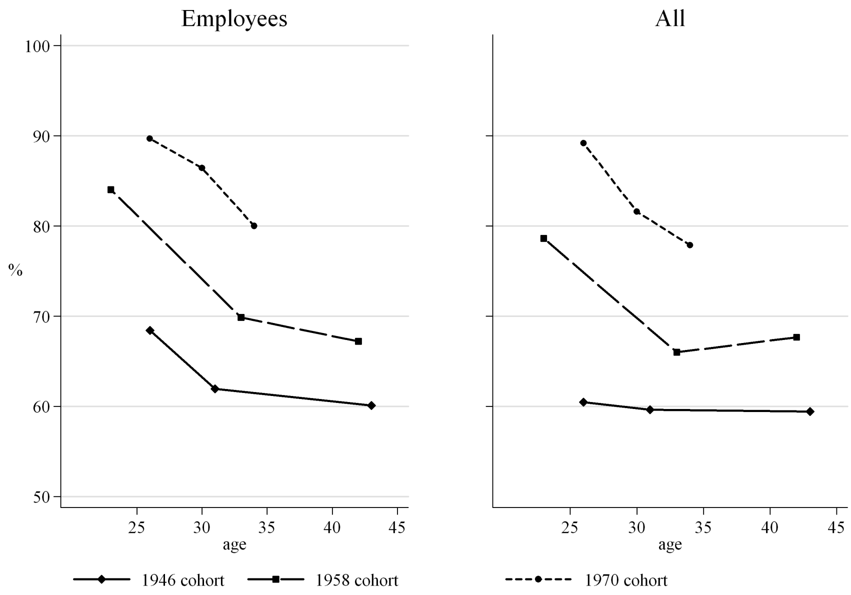

Figure 5.8: Trends in women's median observed pay and median pay opportunities (observed plus imputed potential pay), relative to men's

The analysis presented in this chapter tackled the problem of how to assess changes in women's and men's underlying pay opportunities, in light of the simultaneous and interdependent changes in women's earnings and employment rates. The approach taken was to impute potential wages for individuals not in work. These were imputed using the assumption that the potential wage of each individual not in work would be equal to the actual wage of an individual who was in work, of the same gender, cohort and age and who had similar social, educational and family characteristics and a similar employment history up to that point. The results of this exercise suggest a cross-cohort improvement in women's pay opportunities, relative to men's, even greater than the improvement in the relative pay of working women.

The methods used to take into account the potential wages of non-employees, and the assumptions upon which they are based, are important. The method here was based on the assumption of 'selection on observables', working on the basis that unobserved differences between observably-similar individuals in and not in work are negligible. Some additional work was done to investigate the validity of this assumption for the samples used. The evidence was mixed, with some evidence supporting the assumption of selection on observables, some evidence of possible negative selection into employment amongst women in the two earlier cohorts when in their twenties, and some evidence of possible positive selection into employment for the later cohorts when in their thirties and forties. It is possible that unobserved selection effects could offset some of the observed selection effects that are picked up, which show the reverse trend i.e. negative for the 1946 and 1958 cohort at younger ages and neutral at older ages. In light of this evidence, the effects of accounting for employment selection may be overstated in the imputed data. However, the unobserved selectivity bias would have to be very strong to undermine the main finding of a cross-cohort improvement in women's relative pay opportunities.

The broad picture here is of an improvement in women's position in the labour market in Britain since the 1970s, including an improvement in their underlying pay opportunities relative to men's. The next chapter turns to the question of the unequal remuneration of women's and men's skills in the labour market, focusing on a narrower concept of labour market inequality and the measurement issues involved in quantifying this. Chapter 7 analyses the same cohort samples that have been used in this chapter, focusing on differences in the pay of women and men with similar levels of education and employment experience.

Appendix 5: Figures

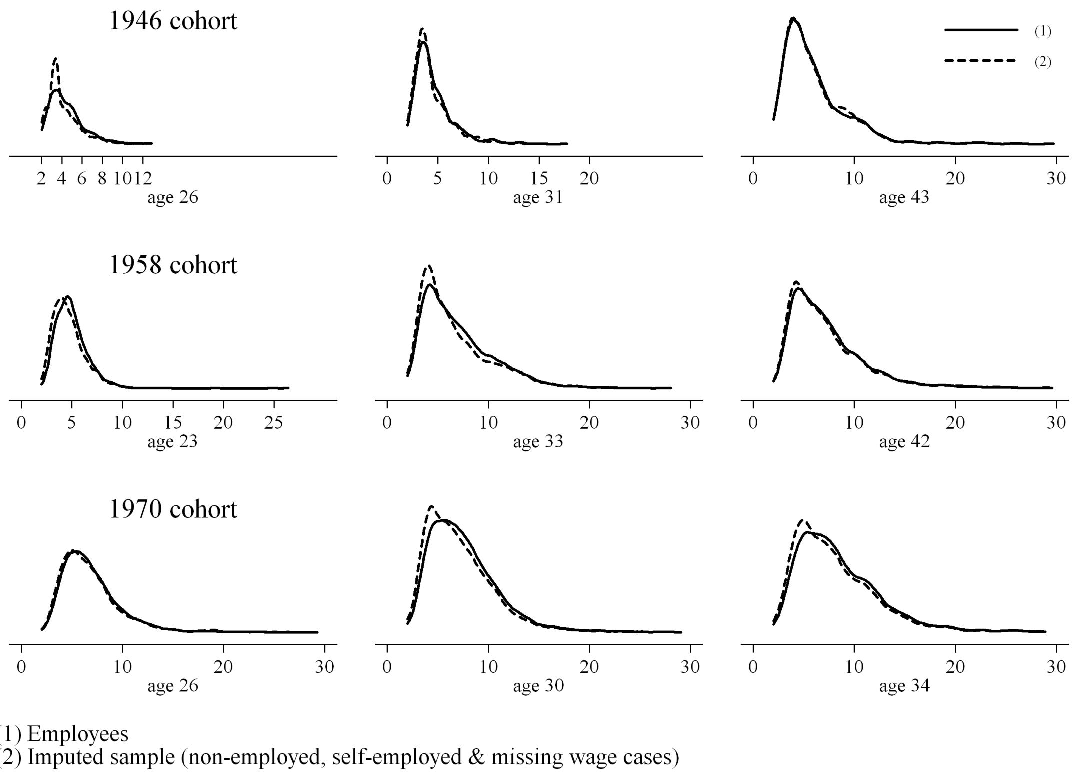

Figure 5.9: Observed and observed-plus-imputed potential wage distributions for women, by survey

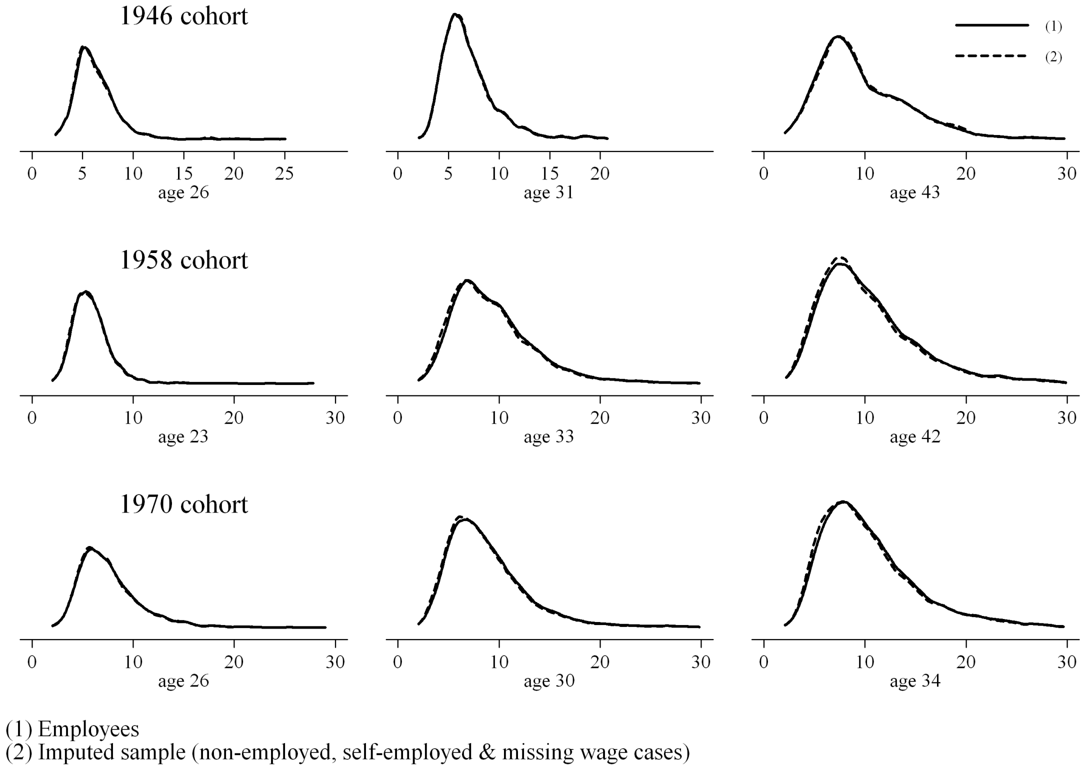

Figure 5.10: Observed and observed-plus-imputed potential wage distributions for men, by survey

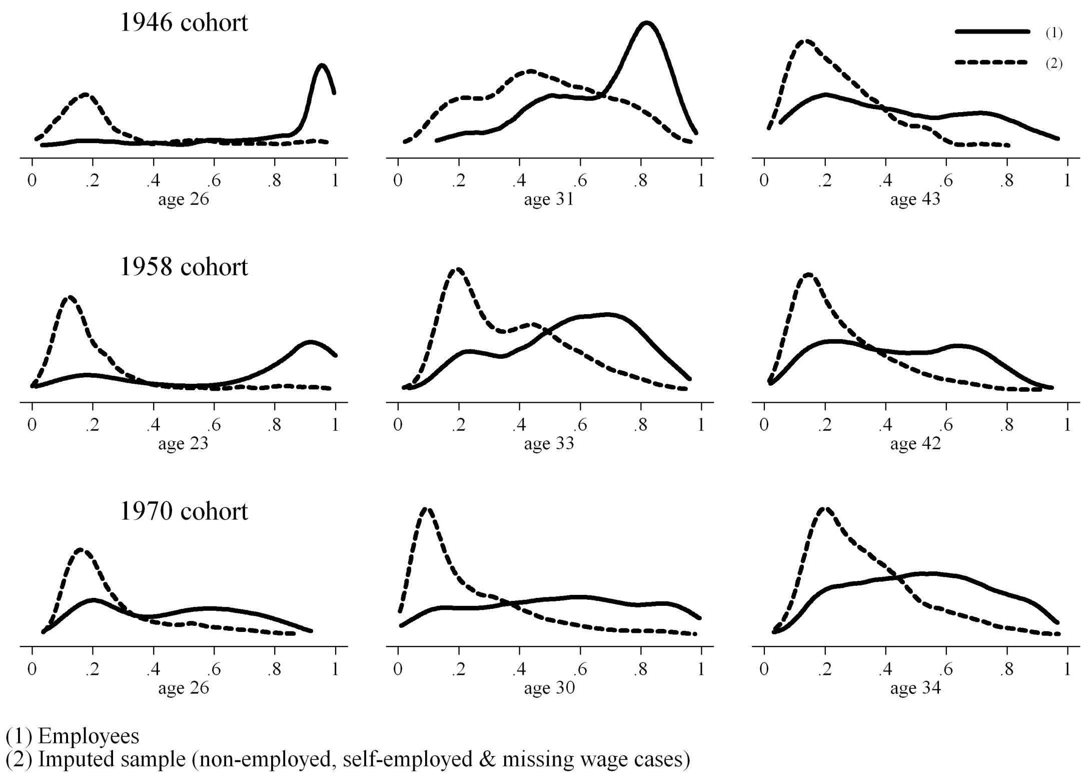

Figure 5.11: Distribution of propensity scores for female employees (with observed wage) and female non-employees (plus self-employees and employees with a missing wage) at each sample

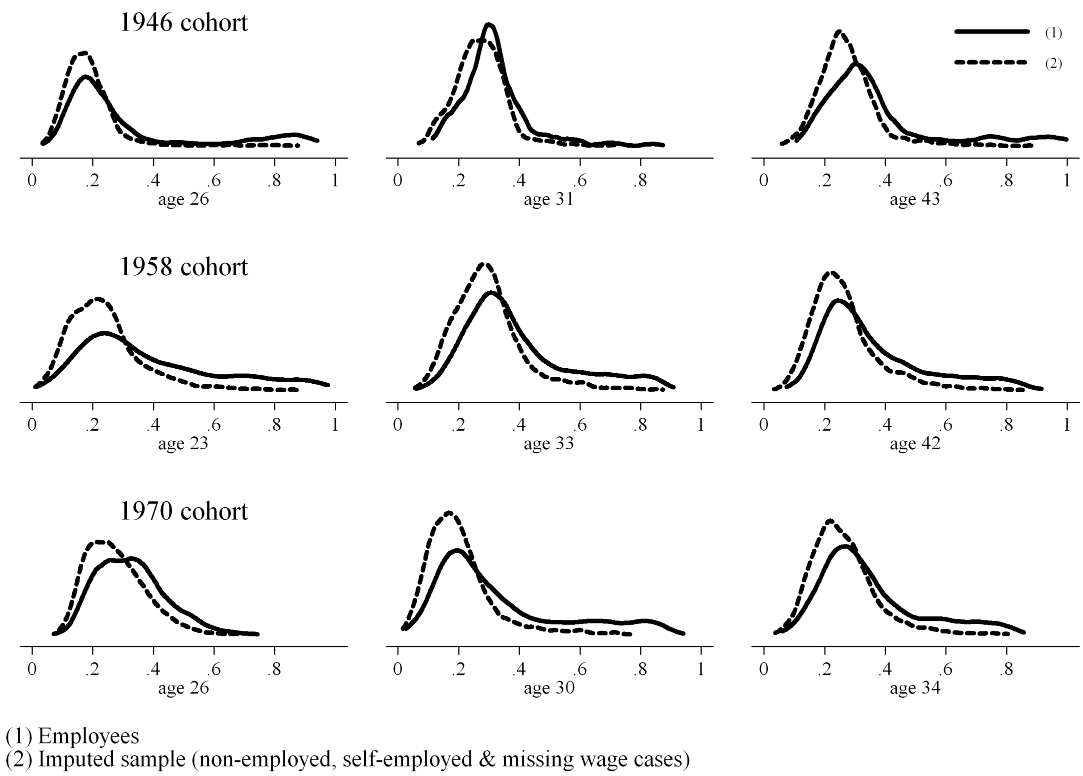

Figure 5.12: Distribution of propensity scores for male employees (with observed wage) and male non-employees (plus self-employees and employees with a missing wage) at each sample

Chapter 6

Methodological literature II: Measuring unequal treatment in the labour market

This chapter is concerned with the measurement of unequal treatment of women and men in the labour market.[1] The concept of unequal treatment here is from the conventional economic literature, which defines this in terms of differences in the pay (or other treatment) of equally productive individuals. Applied to the question of gender inequality, the implication is that differences in women's and men's pay stem partly from unequal treatment and partly from decisions about education, work and family life that contribute to gender differences in productivity. The labour market theories that form the basis for this view of inequality were surveyed in chapter 2. This chapter focuses on the measurement problems associated with quantifying unequal treatment and productivity.

The first part of the chapter sets out the standard decomposition method for measuring discrimination (Oaxaca, 1973; Blinder, 1973). The fraction of the gender pay gap associated with systematic gender differences in productive characteristics is attributed to productivity differences. The unexplained fraction, associated with gender differences in the wage returns to those characteristics, is attributed to discrimination. Semi-parametric methods which directly compare the pay of matched sub-groups of

[1] The terms 'unequal treatment' and 'discrimination' are used interchangeably in this chapter. The term 'unequal treatment' is useful because it is not associated with specific forms of discrimination e.g. those set out in legislation.

women and men are described and briefly compared to traditional parametric methods.

The second part of the chapter focuses on some of the problems with measuring productivity and the associated biases in the estimated effects of discrimination on pay. The latter depend critically upon the individual characteristics that are selected as measures of productivity. These contribute, directly in the decomposition model and implicitly in the matching model, to the explained fraction of the pay gap. It is standard in these models to use educational attainment and length of time in employment as measures of individual productivity. Measurement problems arise from the potential correlation of employment experience with other, discriminatory, influences on pay, such as negative signalling associated with taking career breaks. Further, the interdependence (endogeneity) of higher earnings and employment decisions leads to possible biases in the estimated wage returns to experience. Different ways of incorporating estimated selectivity biases into estimates of unequal treatment depend upon different assumptions about the relationship between unobserved influences on wages and productivity. Different methods for estimating trends in unequal treatment also depend upon the assumed relationship between observed individual characteristics and underlying individual productivity and how to compare like with like over time.

The third part of the chapter briefly discusses cumulative, feedback effects of discrimination over the life-cycle. This is the idea that discrimination experienced at a certain point in the life-cycle, e.g. women at younger ages, may be masked by its effects on subsequent employment decisions and productivity, such that gender pay gaps later in life are wholly accounted for. The final part of the chapter summarises existing estimates of unequal treatment of women and men in the British labour market over the last four decades, drawing attention to the measurement strategies used.

6.1 Methods for quantifying discrimination

6.1.1 The standard decomposition method

The statistical decomposition of gender pay gaps into productivity-related and discriminatory elements is common in the empirical measurement of pay discrimination. This is based on a method proposed by Oaxaca (1973) and Blinder (1973).

Oaxaca (1973) originally proposed the method to quantify Becker's market discrimination coefficient i.e. 'the percentage wage differential between two types of perfectly substitutable labor'. In line with Becker's theory, Oaxaca identified 'culture, tradition and overt discrimination' as factors which restrict women's participation in paid work. Where the two groups are not perfect substitutes, the discrimination coefficient can be generalised as the difference between the observed wage ratio and the hypothetical wage ratio that would exist in the absence of discrimination:

$$D = \frac{E(w_m/w_f) - E(w_m^c/w_f^c)}{E(w_m^c/w_f^c)} \tag{6.1}$$

where $E(w_m/w_f)$ is the expected value of observed ratio of men's to women's wages (typically the ratio of mean wages) and $E(w_m^c/w_f^c)$ is the expected value of the hypothetical non-discriminatory male to female wage ratio. In a competitive frictionless labour market, the non-discriminatory ratio would be equal to the expected ratio of male to female marginal products $E(\mathrm{mp}_m/\mathrm{mp}_f)$. Equation 6.1 can also be written $D = [E(w_m/w_f)/E(w_m^c/w_f^c)] - 1$

The essence of the decomposition approach is to estimate wage equations separately for women and men and to rearrange the difference in mean log wages into two terms; the first relating to differences in women's and men's characteristics (for some fixed set of prices) and the second relating to differences in the wage returns to those characteristics (for some fixed set of characteristics). The second of these terms forms the basis for an estimate of the discrimination coefficient, D.

The wage equations, estimated separately for women and men, have the log-linear forms:

$$\ln(w_f) = X_f'\beta_f + u_f \tag{6.2}$$

for men; and

$$\ln(w_m) = X_m'\beta_m + u_m \tag{6.3}$$

for women.

w is the hourly wage, X is a vector of individual characteristics, β is a vector of coefficients and u is an individual error term. The expected log wage for women is $E[\ln(w_f)] = \bar{X}_f'\beta_f$ and for men, it is $E[\ln(w_m)] = \bar{X}_m'\beta_m$.

Using the properties of natural logarithms, the discrimination coefficient, D, can be written as:

$$\ln(D+1) = (E[\ln(w_m)] - E[\ln(w_f)]) - (E[\ln(w_m^c)] - E[\ln(w_f^c)]) \tag{6.4}$$

2

If men's wage structure, represented in coefficients in β_m, were taken to be the hypothetical non-discriminatory wage-structure (β^c), this would imply that men's wages would not change with a shift to a hypothetical, non-discriminatory labour market. In equation 6.4, $E[\ln(w_m)] = E[\ln(w_m^c)]$, so these two terms drop out of the equation. For women, their expected wage in a non-discriminatory labour market would be given by $E[\ln(w_f^c)] = \bar{X}_f'\beta_m$. Their expected actual wage would be $E[\ln(w_f)] = \bar{X}_f'\beta_f$. Equation 6.4 can be re-written as:

$$\ln(D+1) = \bar{X}_f'\beta_m - \bar{X}_f'\beta_f \tag{6.5}$$

.

A different route to the same result is to rearrange the raw log differential into two terms:

$$E(\ln(w_m) - \ln(w_f)) = \underbrace{\beta_m(\bar{X}_m - \bar{X}_f)}_{\text{explained}} + \underbrace{\bar{X}_f'(\beta_m - \beta_f)}_{\text{unexplained}} \tag{6.6}$$

where the second term is the part of the log wage differential that is not accounted for by differences in men's and women's characteristics. This is equal to $\ln(D+1)$ in 6.5.

Alternatively, if women's wage structure were assumed to be the non-discriminatory wage structure, represented in coefficients in β_f, we would get:

$$\ln(D+1) = \bar{X}_m'\beta_m - \bar{X}_m'\beta_f \tag{6.7}$$

Oaxaca (1973) estimated D using both expressions, on the basis that the non-discriminatory wage structure β^c would lie somewhere in between men's and women's observed wage structures, β_m and β_f. Cotton (1988) and Neumark (1988) suggested measures based on alternative hypothetical non-discriminatory wage structures, which lie somewhere in between the actual male and female wage structures. Oaxaca and Ransom (1994) pointed out that the different decompositions based on different assumed non-discriminatory wage structures can be represented as choices between different weighting matrices Ω in:

[2]In a finite sample, the arithmetic mean of the log wage is equivalent to the geometric mean of the actual wage.

$$\beta^c = \Omega\beta_m + (I - \Omega)\beta_f \tag{6.8}$$

where β^c is a vector of coefficients representing the non-discriminatory wage structure and I is an identity matrix, such that $\beta^c = \beta_m$ when $\Omega = I$. This gives the general form for the decomposition:

$$\ln(D+1) = \bar{X}'_m(\beta_m - \beta^c) + \bar{X}'_f(\beta^c - \beta_f) \tag{6.9}$$

.

6.1.2 Semi-parametric methods

If the relationships between wages and characteristics are very non-linear, the wage structure for a group may not be adequately described by the log-linear wage equations (in equations 6.2 and 6.3). The general, non-parametric wage function can instead be written:

$$w = F(X, g, u) \tag{6.10}$$

where $F(.)$ is some unspecified function and g indicates gender.

Semi-parametric methods for analysing differences in outcomes between groups are based around the idea of matching and weighting samples to compare groups with similar distributions of characteristics in X. This method can be used to get a direct estimate of the percentage wage differential for groups of women and men with similar distributions of characteristics:

$$D = E[(w_m/w_f)|X)] - 1 \tag{6.11}$$

where X represents a fixed distribution of characteristics such that $E[(w^c_m/w^c_f)|X] = 1$.

Matching groups using the propensity score (discussed in chapter 4, section 4.2.2 and chapter 3, section 3.2.1) implies that equation 6.11 can be estimated as:

$$D = E[(w_m/w_f)|p)] - 1 \tag{6.12}$$

where p is the propensity score, which in this application reduces X to a scalar that weights the characteristics in X according to how strongly they differ across women and men.

The choice of which set of characteristics X to use to compare the pay of groups of women and men matters if unequal treatment varies across the distribution(s) of X. Comparing women's average pay to that of a weighted or sub-sample of men with the same characteristics (X_f) implies that it is men's wages that would not change if the labour market were non-discriminatory. This is equivalent to the estimate given by the decomposition given in equation 6.5. The choice of X in the matching model is equivalent to the selection of β^c in the decomposition model.

Angrist and Pischke (2009) have argued that parametric regression methods can be viewed as a particular form of weighting estimator and that differences in results from matching and regression models are unlikely, on their own, to be of empirical importance. They have also pointed out that extrapolation across cells is involved in both matching and regression methods[3] and that both require some form of practical compromise in the exercise of comparing like-with-like.

6.2 Measuring productivity

The main difficulty with trying to measure the difference in pay for equally productive individuals is that we can't directly measure individual productivity. Individual labour productivity can be defined broadly as the economic value of the contribution that an individual makes (or is able to make) toward the production of some good or service. In the conventional economic analysis of discrimination, it is typically differences in the pay of equally productive individuals that is the focus of analysis, rather than differences in the pay of individuals working in jobs that require the same level of skill. This is because most theories of discrimination encompass barriers to access to certain jobs and levels of seniority for discriminated-against groups, implying under-use of their skills (see chapter 2, section 2.2).

[3] In matching methods, extrapolation may be from very sparsely populated cells i.e. predicted counterfactual outcomes for one group may be based on actual outcomes for a very few individuals in the matched group.

6.2.1 Selection of variables to measure individual productivity

In his original paper, Oaxaca (1973) pointed out that a theoretical stance was implied in the selection of characteristics used to measure productivity:

> It is clear that the magnitude of the estimated effects of discrimination crucially depends upon the choice of control variables for the wage regressions. A researcher's choice of control variables implicitly reveals his or her attitude toward what constitutes discrimination in the labor market. If it were possible to control for virtually all sources of variation in wages, one could pretty well eliminate labor market discrimination as a significant factor in determining wage differentials by sex (or race)... The result is that whatever the wage differential observed, it is completely justified on the grounds of alleged productivity differences. The other extreme is to control for virtually nothing and thereby minimize the role of productivity differences. This is tantamount to declaring at the outset that the two labor inputs are near perfect substitutes and therefore attributing virtually all of the observed wage differential to labor market discrimination.

The decomposition method proposed by Oaxaca (1973) was originally presented as a way of measuring Becker's market discrimination coefficient. In contrast, empirical applications of this method have explicitly viewed labour market discrimination as a broader, mixed phenomenon, including monopsonistic behaviour and statistical discrimination by employers (see for example Joshi and Paci, 1998 on gender discrimination and Ermisch and Wright, 1993 on the lower pay of part-time employees). Which of these models of discrimination is assumed to be the right one affects the choice of control variables.

The starting point for most empirical studies is Mincer's specification of the log wage equation, in which the log wages are modeled as a linear function of years in formal education and years of employment experience, plus a quadratic of the experience term. These covariates are treated as measures of human capital, that are presumed to affect individual productivity and, via productivity, individual wages.

$$\ln(w) = \beta_0 + \beta_1 \text{educ} + \beta_2 \text{exp} + \beta_3 \text{exp}^2 + u \tag{6.13}$$

Mincer and Ofek (1982) proposed a slightly different specification for women, separately modelling periods in and out of employment, which are interpreted as periods of investment in and depreciation of human capital respectively:

$$\ln(w) = \beta_0 + \beta_1\text{educ} + \beta_2\text{exp}_0 + \beta_3\text{exp}_1 + \beta_4\text{home}_0 + \beta_5\text{home}_1 + u \tag{6.14}$$

where exp_0 represents past experience (prior to the most recent spell of non-participation), exp_1 represents recent experience, home_0 represents past non-participation (prior to the most recent spell in work) and home_1 represents years of non-participation since the most recent spell in employment. Positive values for exp_1 and home_1 will not occur for the same observation.

Measures of occupational status and group have been included in many empirical studies of the gender pay gap (e.g. Anderson et al., 2001). In many countries, women and men remain quite segregated in the labour market and rates of pay tend to be lower in female-dominated occupations. The argument for including occupational measures in decomposition models to measure unequal treatment is that the segregation of women and men into different occupations, and the related differences in pay, are non-discriminatory. Under this view, women choose occupations that are lower-paid, but which are also less demanding and which offer more flexibility, enabling a combination of childcare and paid work. The alternative point of view is that women are segregated into different occupations partly through discriminatory barriers to entry into certain fields of work and to promotions within a particular field. Further, lower rates of pay in female-dominated occupations may be viewed as the outcome either of the oversupply of women's labour and/or of monopsonistic behaviour by employers. If occupation is viewed as an outcome of labour market discrimination rather than individual choice or productivity, it does not belong in the regression or matching model. More generally, it can be thought of as 'bad control' that is itself an outcome of the phenomenon being investigated (see Angrist and Pischke, 2009, chapter 3, section 3.2.3).

6.2.2 The relationship between earnings and employment experience

The empirical evidence on experience-earnings profiles for the US and the UK suggest that the simple specifications proposed by Mincer and Polachek (1974) and Mincer and Ofek (1982) do not fully capture the variation in this relationship (Murphy, 1993;Manning, 2000; Robinson, 2003a). There is considerable evidence on the long-term 'scarring' effects of unemployment on future earnings and job prospects, including for members of the 1958 cohort (Arulampalam, 2001; Gregory and Jukes (2001); Gregg (2001)). Using data from the British Household Panel Study (1991-1996), Swaffield (2007) found that the negative effects of periods out of employment differ by type of activity.

It is standard to include years of employment experience and job tenure as measures of productivity in wage equations. The basis for doing so is that individuals enhance their general human capital through years spent in employment and develop their specific skills through years spent in the same job. The positive associations between employment experience and length of time in the same job, on the one hand, and wages, on the other, are typically interpreted in this light. However, this interpretation has been challenged and alternative interpretations have been offered, focusing on the role of job search and asymmetric information in the labour market.

An alternative interpretation to the human capital model, arising from a monopsonistic/job search view of the labour market, is that the observed correlation between employment experience and earnings is not primarily through the effects of experience on productivity, but via job mobility. Years spent in employment are correlated with job mobility, and it is the effects of job mobility on earnings that is picked up in the wage model (e.g. see Manning, 2000 or Manning, 2003, chapter 6). On this view, the longer the time spent in and out of the labour market overall, the greater the opportunities to invest time in searching for a good job and the greater the likelihood of receiving a good wage offer. Further, where a good match is achieved, a person is likely to stay in the job, also giving rise to a positive association between earnings and job tenure.

Manning (2003) has drawn attention to empirical evidence that is not consistent with a straightforward human capital interpretation of earnings-experience profiles. Using data for the US Current Population Study (CPS), he found that earnings losses associated with involuntary job loss are greater for those who have been in employment for longer, holding fixed the time spent in the same job. He pointed out that this pattern of earnings losses is not consistent with a human capital interpretation, since the wage returns to *general* human capital should not be affected by job loss. Focusing on the gender gap in pay, Manning (2003) further pointed out that the wage returns to job tenure are higher for women than for men. He argued that this pattern is again not consistent with a simple model of returns to firm-specific training.

A third interpretation of the relationship between earnings and employment experience draws on the idea of asymmetric information in the labour market. Under this interpretation, employers use positive and negative signals when making decisions about employment and wage offers. Unemployment and periods out of the labour market may be treated as negative signals about an individual's likely productivity and work commitment. Albrecht et al. (1999) used Swedish panel data to re-examine the relationship between career interruptions and subsequent earnings. Using fixed-effects estimation to allow for unobserved differences across individuals, they found that the negative effects of parental leave and household time on wages are significantly larger for men than for women.[4] The authors estimated a 6 per cent annual loss of earnings for men taking parental leave, compared to a 2 per cent annual loss for women.[5] The authors argued that the larger negative effect on men's (vs women's) earnings from taking time out of work to raise children was more consistent with a signaling model than with a human capital model.

For Britain, estimated female returns to experience are found to be lower for part-time vs full-time employment (Robinson, 2003a; Swaffield, 2007). Using longitudinal data from the New Earnings Survey Panel Dataset (NESPD), Connolly and Gregory (2009) have also drawn attention to the long-term negative effects on earnings of periods in part-time work. They pointed out that moving from full-time into part-time work may represent a permanent decline in individuals' work commitment and productivity or may be wrongly interpreted by employers as signalling such a decline. Either way, the result would be a relative loss of earnings associated with the change. Ermisch and Wright (1993) estimated wage functions for women in full-time and part-time jobs using data from the 1980 Women and Employment Survey. They found evidence that part-timers receive lower wage offers than similarly qualified full-timers and argued that the lower mobility of women in part-time jobs may also be part of the explanation.

The predictions about the effects of employment experience (and unemployment) on future earnings offered by the different labour market theories are not very different. As a consequence, it is difficult, on the basis of the empirical evidence, to choose between them. If the search and/or signalling models are right (or partly right) though, this implies that employment experience and job tenure are not just measuring productivity. Further, the positive effects of job mobility on pay can be seen as a consequence of employers' monopsonistic market power i.e. their ability to pay *less* mobile individuals less than their economic value. From this point of view, gender differences in pay associated with differences in job mobility could legitimately be considered to be discriminatory. Similarly, employers' less favourable treatment of potential employees who have been out of work or who have spent long periods in part-time work, not in proportion to

[4]Unlike in other countries, the authors point out that Sweden has a well-established generous system of 15 months statutory parental leave, which extends to fathers, with benefits set at 75 per cent of prior earnings for most of the period.

[5]They also divided the analysis by public and private sector to check that the lower rate of earnings loss for women was not related to women's greater representation in the public sector.

decreases in their skills or commitment, could also be viewed as discriminatory.

6.2.3 Interdependence (endogeneity) of earnings and employment experience

A further problem with treating the positive correlation between employment experience and earnings as a measure of the wage returns to human capital is that the correlation may also pick up the positive association between past and current earnings. Length of time in employment is the outcome of past employment decisions. As discussed in chapter 3, these decisions are responsive to potential earnings. As a result, the partial correlation between employment experience and current earnings may pick up the correlations between past and present earnings, as well as the direct causal effects of employment experience on current pay. In this context, employment experience is described as being endogenous in the wage equation and, as a consequence, the estimated wage returns to experience are biased. The wage equation can be written:

$$\ln(w_t) = X_t'\beta + \beta_1 \mathrm{exp}_t + \beta_2 \ln(w_{t-1}) + u_t \tag{6.15}$$

where exp_t is a measure of current employment experience (at time t), β_1 is the true wage return to employment experience and $\ln(w_{t-1})$ is the log of the past wage (at time $t-1$). If the past wage is omitted from the equation, the estimate of β_1 will be biased. The omitted variables bias formula in this model is:

$$\frac{\mathrm{Cov}(w_t, \mathrm{exp}_t)}{\mathrm{Var}(\mathrm{exp}_t)} = \beta_1 + \beta_2\delta \tag{6.16}$$

where δ is the coefficient from the regression of the past log wage $\ln(w_{t-1})$ on experience exp_t. The size of the bias depends upon the strength of the serial correlation between wages β_2 and the strength of the partial correlation between past wages and subsequent employment decisions (represented in δ).

A common finding across studies and countries is that men's earnings are highly correlated from one year to the next and also over longer periods, suggesting a significant 'permanent' element to life-time earnings, although with greater mobility at younger ages (Atkinson et al., 1992). A similar form of bias may produce an inflated return to time spent in a current job. Analysing longitudinal data from the BHPS (1992-98) the US PSID (1985-97) and the US NLSY (1980-94), Manning (2003) found evidence of substantial 'stayer bias' likely to pollute estimates of the wage returns to job tenure using cross-sectional data. This bias arises in a similar way, from the fact that employees in higher paid jobs are more likely to stay in those jobs. This is consistent with a job search model, in which employees in the better paid jobs are less likely to get higher outside wage offers.

6.2.4 Alternative interpretations of selectivity bias

Similar to the problem of the bias arising from omitted past wages in the wage equation, Heckman (1979) modeled selectivity bias related to non-random employment participation as an omitted variables problem. The two-step method he proposed for estimating selectivity bias was outlined in the appendix to chapter 3. The equation of interest here is:

$$\ln(w^o) = X'\beta + \theta\lambda + u \tag{6.17}$$

where λ is the selection term and θ is the coefficient on the selection term ($\theta = \rho_{uv}\sigma_u$). This indicates the size and significance of the selectivity bias i.e. the partial correlation between the wage offer and the participation decision. The way in which this term is incorporated into the estimate of unequal treatment depends upon the theoretical stance taken about the source and nature of the selectivity bias.

Neuman and Oaxaca (2004) propose three alternative decompositions that incorporate the selection term in different ways, based on three alternative assumptions about the nature of the selectivity bias. The first approach is to remain neutral about the sources of selectivity bias and to net out the gender differential in log pay associated with selectivity bias as a separate term.

A second approach is to interpret the selection term as a measure of unobserved differences in the individual work-orientation and motivation of participants and non-participants that affect their pay. Under this interpretation, gender differences in the value of the selection term ($\lambda_m - \lambda_f$) contribute to the explained part of the gender differential in log pay, whilst differences in the coefficients on the selection term in the male and female wage equations ($\theta_m - \theta_f$) contribute to the unexplained gap.

A third interpretation of the selection term is that it represents discriminatory differences in wage offers for participants and non-participants. Under this interpretation, differences in the value of the selection term and also in the coefficient on the selection term in the wage equation would both be counted toward the unexplained part of the differential.

Dolton and Makepeace (1987b) discuss more fundamental problems with the economic interpretation of the selection term. They point out that the coefficient on the selection term only provides information about the relationship between participation probabilities and wage levels for those individuals who are actually in paid work. It can only be used to infer the expected wages of those not in paid work if it is assumed that they share the same wage equation as individuals in paid work. If, instead, the structure of employment opportunities, as well as the individual characteristics, of non-employees is assumed to differ from those of employees, the interpretation is indeterminate.

6.2.5 Measuring change in unequal treatment over time

The analysis of change in unequal treatment over time compounds the measurement problems already discussed and raises further questions about the relationship between observed characteristics and underlying individual productivity. Work by Juhn et al. (1993) on analysing changes in wage structures have inspired two different approaches to the analysis of gender discrimination in pay.

Juhn et al. (1993) suggested a decomposition of changes in the wage structure into three parts:

- first, the effects of changing observed characteristics for a fixed set of prices (wages);
- second, the effect of changing prices (wages) for a fixed set of characteristics; and
- third, the effects of a changing dispersion of wages not captured in the price (wage) structures for observed characteristics, but instead captured in the spread of the residuals.

Juhn et al. (1993) explicitly interpreted the dispersion in wages, controlling from the observable elements (the residual terms) as 'a distribution of unobservable ability in the population in conjunction with a current market value of this unobservable ability.'

Based on this framework, Dolton et al. (2002) proposed a decomposition of changes in gender wage differentials. Using decomposition 6.5, the change in the estimated discriminatory log differential, Δd (where $d = \ln(D + 1)$), across two time periods, 0 and 1 is:

$$\Delta d = d_1 - d_0 = \underbrace{\bar{X}_{f,1}(\beta_{m,1} - \beta_{f,1})}_{d_1} - \underbrace{\bar{X}_{f,0}(\beta_{m,0} - \beta_{f,0})}_{d_0} \tag{6.18}$$

Dolton et al. (2002) pointed out that the change in the log differential itself could be rearranged into two terms. First, there is a change in women's and men's relative wage structures, represented in the βs. Second, there is a change in the weighting of these wage structures, resulting from any change in women's mean characteristics over time. Dolton et al. (2002) proposed a method to decompose the change in the discriminatory log differential over time, rearranging equation 6.18 into two terms:

$$d_1 - d_0 = \bar{X}_{f,0}\underbrace{((\beta_{m,1} - \beta_{f,1}) - (\beta_{m,0} - \beta_{f,0}))}_{\Delta\beta} + \underbrace{(\bar{X}_{f,1} - \bar{X}_{f,0})}_{\Delta\bar{X}_f}(\beta_{m,0} - \beta_{f,0}) \tag{6.19}$$

The term $\Delta\beta$ represents the change in coefficients over time, whilst the term $\Delta\bar{X}_f$ represents the change in women's average characteristics over time. The second term affects the change in the unexplained differential through altering the weighting of the coefficients $(\beta_{m,0} - \beta_{f,0})$. Dolton et al. (2002) proposed that the change in the unexplained log differential associated with the first of these terms, $\Delta\beta$, should be interpreted as the 'pure' change in discrimination.

Taking this approach a step further, the non-parametric equivalent to this method would be to estimate the trend in gender differentials for some fixed set of characteristics i.e. compare the relative pay of a sub-group or weighted sample with fixed characteristics $\bar{X}_{f,0}$.

There are reasons to be cautious about this approach. First, if women's characteristics have changed over time ($\Delta\bar{X}_f$) in such a way that a greater fraction of them have characteristics which attract more equal treatment (e.g. say more educated women and men were treated more equally), this still represents a reduction in unequal treatment overall, which would not be captured in the term associated just with $\Delta\beta$. Second, general equilibrium effects are important here. It is not clear that it is a sensible thought-experiment to abstract the wage returns (prices) for characteristics from the composition (supply) of characteristics in the population.

A second strand of research inspired by the work of Juhn et al. (1993) has focused on disentangling the effects of a changing wage structure from the effects of changes in characteristics and the wage returns to observed characteristics. Blau and Kahn (2006a) and Harkness (1996) have examined the relationship between changes in the wage distribution and trends in the gender pay gap, for the US and the UK respectively. Blau and Kahn (2006a) adapted the framework proposed by Juhn et al. (1993) to analyse changes in the gender differential in wages, where the male wage structure is used to estimate the changes in characteristics and the changes in wage returns (prices) for those characteristics over time, whilst the unexplained gender gap forms part of the residual term. This approach is also rather problematic, since strong parametric assumptions are required and the broad approach proposed by Juhn et al. (1993) has been criticised on these grounds (Suen, 1997). Blau and Kahn (2001) and Harkness (1996) have also used simpler methods to infer the effects of wage institutions and wage structures on gender pay gaps (across countries as well as over time). There is, however, a question as to whether gender discrimination effects can usefully be abstracted from changes in wage structures and the 'prices' on characteristics that are distributed differently across women and men.

6.3 Cumulative life-cycle effects of unequal treatment

A final problem with measurement strategies which compare the pay of women and men with similar characteristics, at a given point in time, is that those characteristics themselves may be the outcomes of discrimination earlier on in life. For example, women with discriminatory lower wage offers at young ages may respond by reducing their participation in the labour market (Gronau, 1988). In turn, time spent out of the labour market may lower their productivity (leaving aside the measurement issues discussed in section 6.2) and future pay. Oaxaca (1973) acknowledged this limitation of the decomposition method for quantifying discrimination:

> Another difficulty with the residual approach is that it does not take into account the effects of the feedback from labor market discrimination on male-female differences in the selected individual characteristics. The differences could reflect the adaptation of women to the biases of the labor market; yet under the residual approach all differences in the characteristics contribute to a reduction of the wage differential attributable to discrimination. The problem becomes one of how much of the observed differences

in individual characteristics would exist in the absence of discrimination. These very difficult problems have not been dealt with in this study, but they are clearly important in terms of policy prescriptions for narrowing the male-female wage differential.

6.4 Estimates of unequal treatment in the British labour market over four decades

A large number of studies have used decomposition methods to analyse gender differentials in pay. Here, I summarise the empirical estimates from studies using British national datasets over the last forty years. I consider the evidence on changes in unequal treatment in the British labour market over time and changes in unequal treatment over the life-cycle.

The results from ten studies are summarised in table 6.1. Comparing estimates from surveys for different years over the period 1974-2000, the decrease in the raw gender pay gap over time is clear. This also came out clearly from the evidence discussed in chapter 3 (e.g. Blundell et al., 2007) and the cross-cohort trends presented in chapter 4.

Also apparent is a decrease in the unexplained log gap $\bar{X}_f(\hat{\beta}_m - \hat{\beta}_f)$ which is an estimate of $\ln(D + 1)$. For example, comparing the estimate for the 1980 Women and Employment Survey (Wright and Ermisch, 1991) to the estimate for the 1991 survey of the 1958 cohort (Joshi and Paci, 1998, table 5.5) for all women and men, the unexplained log gap fell from 0.229 to 0.244. Looking at the trends for full-time employees from General Household Survey cross-sections, Harkness (1996) estimated an unexplained log differential of around 0.34 in 1974, falling to 0.24 in 1983 and again to 0.20 in 1992-93.

From the results summarised in table 6.1, estimates of unequal treatment appear to be sensitive to three related factors:

1. The estimate is sensitive to the inclusion of employment experience as a control variable and is smaller when this is included (e.g. compare rows 3 and 6, 16 and 17 or rows 18 and 19 of table 6.1);
2. The estimate is sensitive to the inclusion of part-time employees in the sample and tends to be larger if part-time employees are included (e.g. compare rows 9 and 10 or 16 and 18 of table 6.1); and

3. The estimates differ across different age groups and tend to be larger for older employees (e.g. compare rows 13 and 20 for a comparison of same cohort at different ages, or rows 20 and 21 for a comparison of different cohorts in same year - table 6.1).

On the first of these issues, there is further evidence to suggest that gender differences in employment experience are empirically important in accounting for gender gaps in pay. Myck and Paull (2004) found that differences in average levels of experience across women and men accounted for nearly two-fifths of the difference in their average hourly earnings in the UK (using cross-sectional data from the Family Expenditure Survey) and in the US (using cross-sectional data from the Current Population Survey). Using labour market history data from the British Household Panel Study over the period 1991-97, Swaffield (2007) compared a set of estimates that controlled for potential years in the labour market to another set of estimates that controlled for actual labour market participation. She found that gender differences in actual experience were a critical factor and reduced the unexplained pay gap by around 40 per cent (see rows 18-21 of table 6.1)). Manning and Swaffield (2008) also found that gender differences in employment experience contributed to around a quarter of the gender difference in wage growth over the first ten years in the labour market after leaving full-time education.

On the second of these issues, the smaller estimates of unequal treatment for full-time employees is consistent with the evidence of lower returns to all characteristics for part-time employees (Ermisch and Wright, 1993). It is also consistent with the longitudinal evidence on decreases in relative pay associated with changes in employer and occupation that often accompany a move from full-time to part-time working hours (Connolly and Gregory, 2008; Connolly and Gregory, 2009).

On the third of these issues, the larger estimates of unequal treatment for older employees are consistent with the more limited evidence on this. Joshi et al. (2007) found that estimated unequal treatment increased across women and men from the 1958 cohort between the ages of 33 and 42, comparing the full-time working populations at each age. They also estimated an increase in unequal treatment for the sub-sample of women (989 women) who were working full-time at both ages.

6.5 Discussion

This chapter has set out the conventional economic approach to quantifying discrimination in the labour market. The decomposition method proposed by Oaxaca (1973) and Blinder (1973) is still widely used. This is based on a decomposition of the gender pay gap into an explained fraction, which is associated with gender differences in observed characteristics, and an unexplained fraction that is associated with gender differences in the wage returns to those characteristics. Typically, the explained fraction is interpreted as the productivity-related pay gap and the unexplained fraction is interpreted as the discriminatory pay gap.

The chapter has considered the linkages between alternative measurement strategies and theoretical models of labour market discrimination. The critical issue of measuring productivity has been discussed. The selection of individual characteristics to measure productivity and to contribute to the explained (non-discriminatory) fraction of the pay gap, and the theoretical stance implied by different selections, has been discussed. In particular, from the perspective that job search and asymmetric information are important factors in the labour market, the goodness of employment experience as a measure of productivity within wage decompositions is debatable. The theoretical assumptions that form the basis of other measurement decisions have also been discussed, including decisions about how to estimate trends in unequal treatment over time.

The empirical evidence for Britain over the last four decades has been surveyed. The results suggest a decrease in the unequal treatment of women and men in the British labour market between 1974 and 2000. They also suggest variation in unequal treatment by part-time status and age. Finally, the results confirm the empirical importance of employment experience in accounting for a large part of the gender pay gap and, consequently, the sensitivity of estimates of unequal treatment to different assumptions about the goodness (or badness) of employment experience as a measure of individual productivity.

The next chapter estimates trends in unequal treatment across the three birth cohorts. Standard decomposition methods, and semi-parametric versions of these, are used to estimate unequal treatment for the cross-sections of cohort members in employment at each of the surveys. No statistical methods have been used to try and directly estimate and correct for bias arising from unobserved selectivity into employment or from the likely endogeneity of employment experience. Instead, some investigation has been carried out into these problems associated with comparing the pay of women and men with similar employment histories. Further, a second exercise exploits the longitudinal aspect of the data to estimate within-cohort trends in pay for matched sub-samples of women and men with similar qualifications and employment patterns. In comparing across cohorts, early attempts to compare individuals with similar observed characteristics were discarded on the basis that these were likely to be polluted by selectivity bias and measurement error, apart from any theoretical concerns about the validity of this comparison.

Table 6.1: Estimates of unequal treatment from decompositions, by year of survey

	Study	Survey	Year	Sample	Sample size	Controls	Raw gap (a)	Unexplained gap (a)
1	Harkness (1996) (b)	GHS	1974	full-time employees	5,671 men 2,386 women	age, highest qualification	0.408	0.339
2	Harkness (1996) (b)	GHS	1974	full-time employees	5,671 men 2,386 women	as above, plus occupation, region and children	0.408	0.362
3	Zabalza and Arrufat (1985), tables 5.1 and 5.6	GHS	1975	married, all employed	3,984 men 1,368 women	Years in education, potential work experience	0.473	0.460
4	Zabalza and Arrufat (1985)	GHS	1975	as above	as above	years in education, predicted work experience	0.473	0.183
5	Joshi and Paci (1998); Makepeace et al. (1999)	1946 cohort	1977/78	full-time employees age 31/32	1,051 men 263 women	highest qualification, work experience, years in current job, age 11 test scores, region	0.305	0.214
6	Wright and Ermisch (1991)	WES	1980	married, all employed	1,868 men 2,094 women	highest qualification, region, actual work experience	0.398	0.229
7	Harkness (1996) (b)	GHS	1983	full-time employees	4,211 men 1,971 women	age, highest qualification	0.318	0.239
8	Harkness (1996) (b)	GHS	1983	full-time employees	4,211 men 1,971 women	as above, plus occupation, region and children	0.318	0.295
9	Joshi and Paci (1998), table 5.5	1958 cohort	1991	all employees age 33	1,797 men, 1,370 women	highest qualification, age 11 test scores, work experience, region, years in current job	0.337	0.244
10	Joshi and Paci (1998), table 4.5; Makepeace et al. (1999)	1958 cohort	1991	full-time employees age 33	3,098 men 1,421 women	as above	0.167	0.156
11	Joshi and Paci (1998), table 4.5	1958 cohort	1991	full-time employees age 33	1,797 men 866 women	as above, plus firm, job and occupational characteristics	0.183	0.114
12	Joshi et al. (2007), table 3	1958 cohort	1991	full-time employees age 33	3,659 men, 1,704 women	Highest qualification, full-time and part-time experience, years in current job, age 11 test scores, region.	0.163	0.147

Table 6.1: Estimates of unequal treatment from decompositions, by year of survey - continued

	Study	Survey	Year	Sample	Sample size	Controls	Raw gap (a)	Unexplained gap (a)
13	Harkness (1996) (b)	BHPS	1992-93	full-time employees	1,934 men 1,171 women	age, highest qualification	0.221	0.196
14	Harkness (1996) (b)	BHPS	1992-93	full-time employees	1,934 men 1,171 women	as above, plus occupation, region, children and work experience	0.221	0.203
15	Swaffield (2007), table 5	BHPS	1991-97	all employees	5,956 men 6,923 women	highest qualification, part-time worker, years in current job, training, job characteristics (not occupation), potential labour market experience	0.311	0.213
16	Swaffield (2007), table 5	BHPS	1991-97	all employees	5,956 men 6,923 women	as above, replace potential with actual full-time and part-time experience, activities while out of work	0.311	0.130
17	Swaffield (2007), table 5	BHPS	1991-97	full-time employees	10,064 total	highest qualification, part-time worker, years in current job, training, job characteristics (not occupation), potential labour market experience	0.196	0.133
18	Swaffield (2007), table 5	BHPS	1991-97	full-time employees	10,064 total	as above, replace potential with actual full-time and part-time experience, activities while out of work	0.196	0.095
19	Joshi et al. (2007)	1958 cohort	2000	full-time employees age 42	3,856 men 2,270 women	highest qualification, full-time and part-time experience, years in current job, age 11 test scores, region	0.303	0.186
20	Joshi et al. (2007)	1970 cohort	2000	full-time employees age 30	4,120 men 2,730 women	as above	0.082	0.112

(a) The raw gap is the difference in the mean log wage, $\ln(w_m) - \ln(W_f)$. The unexplained log gap, $\bar{X}_f(\hat{\beta}_m - \hat{\beta}_f)$, is intepreted as the measure of unequal treatment. Where the results are presented as estimated ratios (based on log wage models), these have been converted to log difference, using $\ln(w_m) - \ln(w_f) = -\ln(w_f/w_m)$. Where the results have been presented as estimates of D, these have been converted to $\ln(D+1)$ for purposes for comparability. (b) Harkness (1996) has used the expression $\bar{X}_m(\hat{\beta}_m - \hat{\beta}_f)$ using men's mean characteristics to weight and compare coefficients for men and women

Chapter 7

Analysis II: Trends in the unequal treatment of women and men in the labour market

This chapter investigates cross-cohort and life-cycle trends in the unequal treatment of women and men in the labour market, using evidence on differences in pay not accounted for by differences in qualifications and employment experience. There is a large existing literature on this subject and the analysis presented here makes two main contributions:

1. New estimates of unequal treatment add to the existing evidence for the cohort studies (e.g. Joshi and Paci, 1998; Joshi et al., 2007). Data from the 1989 survey of the 1946 cohort and the 2004 survey of the 1970 cohort have not previously been used to investigate gender differentials in pay.

2. Life-cycle trends in the pay gap are explored for similarly-qualified women and men using longitudinal samples of those employed and with an observed wage at each survey. The earnings data have not previously been used longitudinally to look at gender differentials. This contributes to the existing evidence on gender gaps in wage growth over the life-cycle (e.g. Manning and Petrongolo, 2008; Brewer and Paull, 2006).

The main results suggest that unequal treatment has reduced since the introduction of anti-discrimination legislation in the 1970s, but had not disappeared for the youngest cohort by the age of 34 (in 2004). Further, unequal treatment increases over the life-cycle for women who spend periods out of work and in part-time work to raise children, even if they later return to full-time employment.

This chapter is divided into three sections. The first section described trends in education, patterns of full-time and part-time working and employment experience across the three cohorts. The second section presents a set of estimates of the unexplained pay gap, after accounting for the impact of gender differences in education, experience, length of job service, childhood maths and reading ability and region of residence. These are estimated on cross-section samples of employees from each survey and add to the estimates summarised in table 6.1 in the previous chapter. Variation in estimates across samples of all employees and restricted samples of full-time employees are investigated. The sensitivity of estimates to different weights and methods is also explored. The third section investigates trends in the relative pay of similarly-qualified women and men over the life-cycle, using the longitudinal aspect of the data. Wage trends are also estimated for longitudinal samples of women who have worked full-time at each survey age and for women who have not had children by the most recent survey, comparing these to trends for men with similar qualifications.

7.1 Description of trends in education and employment experience

Figure 7.1 shows the percentages of women and men with no or low qualifications (NVQ level 1 or below), with O-level qualifications (NVQ level 2), A-level qualifications (NVQ level 3) and diploma or degree-level qualifications (NVQ level 4 and above).[1] The percentages shown are based on the highest qualification attained by cohort members by age 26 for the 1946 cohort, by age 33 for the 1958 cohort and by age 30 for the 1970 cohort. Percentages are estimated for the samples participating the the study at these ages and the 1946 cohort figures are weighted to account for the stratification of the sample.

Gender differences in educational attainment were marked for the 1946 cohort, but had disappeared for the 1970 cohort by the time they had completed their education. Also striking is the cross-generational increase in the proportion of both women and men who gained formal qualifications of any kind and also in the proportion who gained a

[1] For the wage regression and probit model, the top category is split into two, with separate dummy variables for holding a diploma-level and a degree-level qualifications. See chapter 4 for more details.

diploma or degree-level qualification. For the 1946 cohort, nearly 60 per cent of women and over half of men had no formal or low (below O-level) qualifications. Only 9 per cent of women and a fifth of men had diploma or degree-level qualifications. For the 1970 cohort by age thirty, around a fifth of women and men had no or low qualifications, whilst nearly a third of both women and men held diploma or degree-level qualifications.

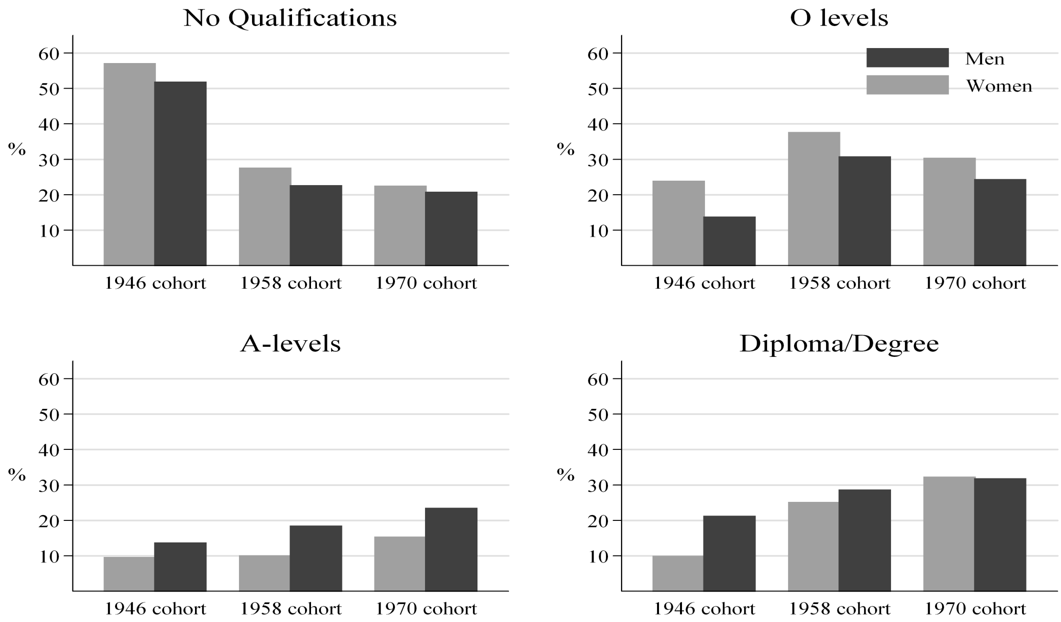

Figure 7.1: Highest qualification attained, percentages by cohort and gender

Women's attachment to the labour force also increased across the cohorts. Figure 7.2 shows the cross-cohort increase in the proportion of women in work when in their twenties and thirties and also the increase in the proportion of women in full-time work at these ages.

Despite the increase in women's employment, there is a gender gap in experience in each cohort. By their early thirties, a substantial gender gap in length of employment experience had emerged for the 1970 cohort. The majority of men had near-continuous employment records, whilst women had more diverse employment patterns. Table 7.1 shows the average number of years spent in work up to each survey age for women and men in each of the three cohorts. For the 1946 cohort, these figures represent the average number of years spent in paid employment since the age of twenty-five. This limits comparisons with the two later cohorts, which cover the average number of years

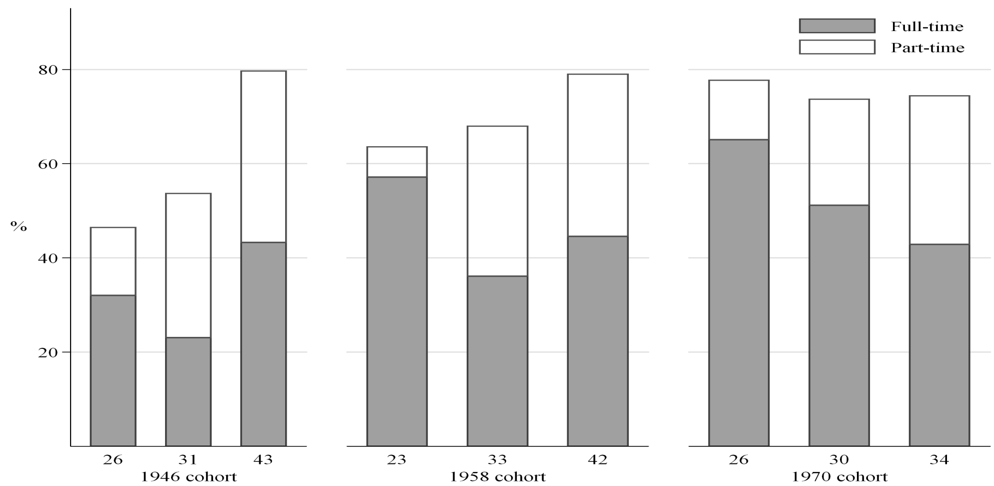

Figure 7.2: % women in full-time and part-time work, by age and cohort

in employment since the age of sixteen (see chapter 4 for a full discussion).

Table 7.1: Mean number of years in work (including self-employment) at each survey, by cohort and gender

	Total exp.			Full-time exp.			Part-time exp.		
	Women	Men	*Diff.*	Women	Men	*Diff.*	Women	Men	*Diff.*
1946 cohort									
Age 31	3.4	5.8	*-2.4*	2.8	5.8	*-3.0*	0.6	-	*+0.6*
Age 43	12.2	17.2	*-5.0*	7.7	17.1	*-9.4*	4.5	0.2	*+4.3*
1958 cohort									
Age 23	4.7	5.5	*-0.8*	4.5	5.5	*-1.0*	0.1	-	*+0.1*
Age 33	10.3	13.4	*-3.1*	8.9	13.4	*-4.5*	1.7	0.1	*+1.6*
Age 42	16.6	21.1	*-4.5*	12.1	20.9	*-8.8*	4.5	0.2	*+4.3*
1970 cohort									
Age 26	5.7	5.1	*+0.6*	5.1	5.0	*+0.1*	0.5	0.1	*+0.4*
Age 30	9.3	10.6	*-1.3*	7.9	10.4	*-2.5*	1.3	0.2	*+1.1*
Age 34	12.5	14.6	*-2.1*	10.1	14.3	*-4.2*	2.5	0.3	*+2.2*

Part-time working has remained an important feature of women's working careers in each of the three cohorts. A substantial fraction of women's employment experience was gained in part-time jobs rather than in full-time jobs (table 7.1). The apparent decrease in part-time working across the cohorts (figure 7.3) should be interpreted with some caution. The figure for the 1970 cohort by age 34 is unlikely to represent the full, future role that part-time working will play in the working lives of this cohort. On the other hand, the figure may also be slightly understated for the 1946 cohort, since it misses out part-time working before the age of 25 (if this was followed by full-time working from age 25 to 43).

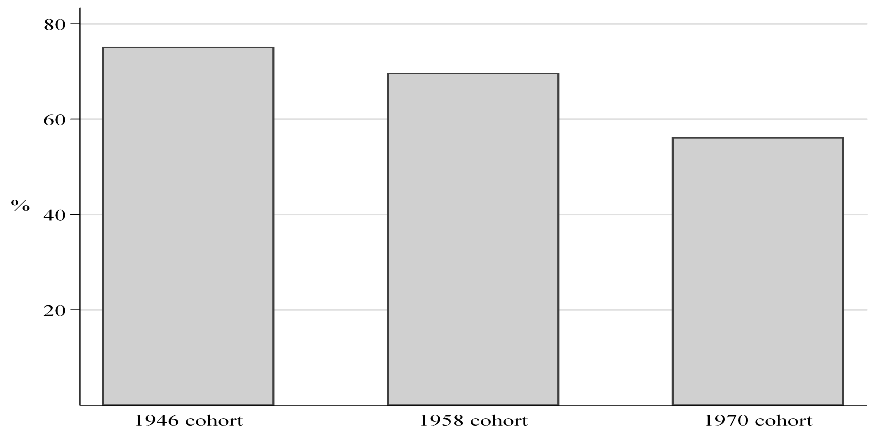

Figure 7.3: % women who have worked part-time up to most recent survey (1)

(1) The most recent surveys used in the analysis are the 1989 survey of the 1946 cohort (age 43), the 2000 survey of the 1958 cohort (age 42) and the 2004 survey of the 1970 cohort (age 34).

Gender differences in job tenure (see tables 7.2, 7.3 and 7.4) follow a similar life-cycle pattern to gender differences in employment experience. Employed women are likely to have been in their current job for fewer years than employed men in each survey and the gender difference increases with age. Unlike employment experience though, there has not been a marked cross-cohort decrease in the gender gap in job tenure.

7.2 Estimates of unequal treatment for cross-section samples of employees

The effects of unequal labour market treatment of women and men on their relative pay are quantified here as differences in log pay, not accounted for by differences in qualifications and experience. This differential $d = \overline{\ln(w_m)} - \overline{\ln(w_f)}$ is an estimate of $\ln(D + 1)$ (see chapter 6). The estimates of the unexplained gap have been left in log form in order to permit comparisons with estimates of unequal treatment reviewed in the last chapter. Note that gender comparisons based on the difference in mean log wages (the exponent corresponds to the geometric mean of wages) differ slightly from comparisons based on relative median wages, which were the focus of chapter 5). However, the statistics are quite close and both place less weight on the extremes of the distribution than arithmetic means.

This section presents estimates from samples of employees at each survey, using the data in a cross-sectional rather than a longitudinal way. The effects of unequal treatment on pay are estimated separately for the whole sample of employeees at each survey and for restricted samples of full-time employees. The sensitivity of these estimates to: 1) the method used to estimate individual counterfactual non-discriminatory wages (wage regression vs. propensity-score matching); and 2) differences in the reference sample used to weight and aggregate these.

7.2.1 Methods and data

Standard decomposition methods and matching methods have been used to construct alternative sets of counterfactual wages. In the standard decomposition method, the counterfactual wage for each individual is their predicted wage from a log-linear wage model estimated for the opposite sex. The predicted wage is intended to represent the wage that they would be paid if their characteristics were remunerated at the same rate as someone of the opposite sex.

For propensity score matching, the counterfactual wage for each individual is the actual observed wage of another individual of the opposite sex who has a similar propensity score. The propensity score is a scalar representing gender differences in characteristics, estimated from a probit model. Each characteristic is weighted in the score by how strongly it discriminates between the female and male samples.[2] For a detailed

[2]The values of individual covariates are multiplied by the marginal effect of these on the propensity

description of propensity score matching, see chapter 5.

Both models were estimated using Stata. The Stata program 'oaxaca' (Jann, 2008) was used to calculate standard errors associated with the decomposition estimates of unexplained log gap in pay. The program 'psmatch2' (Leuven and Sianesi, 2003) was used for propensity-score matching. For both sets of estimates, standard errors were calculated analytically using the assumption that the regressors (covariates) are fixed.[3]

The sensitivity of estimates of unequal treatment to the reference sample selected as the basis for comparison was explored. In the decomposition models, the different estimates come from using different reference group means to weight gender differences in wage model parameters. In the matching models, the different estimates come from weighting observed individual wages to match the distribution of propensity scores in the reference (treatment) group.[4]

Estimates for samples of all women and men employees were compared to those for restricted samples of full-time employees only. For the employee samples, estimates were adjusted for gender differences in length of employment experience, but not for whether this experience was gained in full-time or part-time jobs. For the full-time employee samples, the estimates were adjusted for differences in full-time and part-time experience. Consequently, comparing across the employee and full-time employee samples for the same set of weights (i.e. the same reference sample) gives an idea of the combined effects of: a) differences in parameters in wage models for full-time women vs. all employed women; and b) the importance of distinguishing the effects of full-time experience and part-time experience on pay for women and men.

7.2.2 Cross-section samples

The data used for the analysis in this chapter come from the same samples used for the analysis in chapter 5. The cross-section samples include all women and men who responded to a given survey. The distribution of women and men across economic activity categories at each survey were shown in tables 5.1 and 5.2 of chapter 5.

score (estimated by probit regression). Note that tables 7.7 show the probit coefficients, rather than the marginal effect.

[3] Jann (2008) has pointed out that sthis assumption reduces the standard error less for the unexplained term (i.e. the estimate of unequal treatment which forms the focus of the present analysis) than the explained component of the raw log differential.

[4] In the matching literature, this is referred to as the average treatment effect on the treated group (ATT). Variation in estimates by across different reference samples (weightings) gives an idea of how much unequal treatment of women and men varies by labour market position and skills.

Table 7.2 shows the means of variables for the core samples. These samples include all individuals who responded to a given survey, excluding only those for whom either educational or employment experience variables were missing, or who had a missing wage if they were employed.[5] The change in the means of reading and maths scores (taken at age 11) across the adult surveys is a useful indicator of the effects of attrition and survey non-response on sample representativeness. The small increase in the average scores with age is evidence of a small deterioration of representativeness, with higher attrition amongst individuals with low childhood ability scores. The higher means for the 1996 survey of the 1970 cohort also signal the non-random response to the postal survey conducted at this age. See chapter 4 for a full discussion of the effects of survey non-response, attrition and missing data on sample representativeness.

Table 7.3 shows the means of these variables for the cross-section samples of employees at each survey. The difference between these and the sample means for the whole samples (including non-employees) show the effects of non-random selection into employment amongst women, which was explored in chapter 5.

Table 7.4 shows the means of the variables used for the cross-section samples of full-time employees at each survey. Women who work full-time in their twenties or thirties tend to have higher maths and reading scores and to be more highly qualified and experienced than women in part-time work (who are included in the employee sample) and women not in work (who are included in the whole sample). Nearly all employed men work full-time and the differences in variable means across the male employee and male full-time employee samples are negligible.

[5]The analyses were run on samples including predicted values for employees with missing wages and the results were unchanged.

Table 7.2: Variable means for cross-section samples, including non-employed (1)

	Women									Men								
	1946 cohort (2)			*1958 cohort*			*1970 cohort*			*1946 cohort*			*1958 cohort*			*1970 cohort*		
	26	*31*	*43*	*23*	*33*	*42*	*26*	*30*	*34*	*26*	*31*	*43*	*23*	*33*	*42*	*26*	*30*	*34*
Maths score (3)	-0.09	-0.04	-0.04	+0.06	+0.05	+0.05	+0.13	+0.02	+0.06	-0.12	-0.08	-0.05	+0.06	+0.11	+0.10	+0.24	+0.10	+0.14
Reading score (3)	-0.07	-0.03	-0.01	+0.04	+0.07	+0.06	+0.22	+0.10	+ 0.14	-0.12	-0.10	-0.07	+0.03	+0.08	+0.08	+0.15	+0.01	+0.05
Missing score (3)	0.10	0.10	0.10	0.14	0.14	0.14	0.24	0.25	0.25	0.12	0.11	0.10	0.14	0.14	0.14	0.25	0.25	0.25
O-level (4)	0.24	0.25	0.25	0.30	0.38	0.31	0.41	0.30	0.26	0.14	0.15	0.13	0.30	0.31	0.28	0.33	0.24	0.21
A-level (4)	0.09	0.09	0.10	0.22	0.10	0.13	0.11	0.15	0.16	0.13	0.12	0.16	0.22	0.18	0.18	0.14	0.23	0.24
Diploma (4)	0.07	0.07	0.08	0.09	0.16	0.16	0.10	0.15	0.18	0.11	0.12	0.12	0.09	0.15	0.17	0.09	0.13	0.15
Degree (4)	0.03	0.03	0.08	0.10	0.13	0.14	0.19	0.17	0.20	0.10	0.10	0.14	0.10	0.14	0.16	0.23	0.19	0.21
Years in current job (5)	1.06	1.98	4.25	2.65	2.69	4.80	2.80	2.95	3.42	2.50	3.62	7.15	2.65	4.73	7.70	3.11	4.12	5.51
Years in work	0.47	3.12	12.15	5.50	10.21	16.59	5.69	9.26	12.48	0.95	5.79	17.25	5.50	13.44	21.17	5.09	10.06	14.58
London or SE (6)	0.31	0.30	0.30	0.10	0.31	0.29	m	0.30	0.30	0.30	0.30	0.29	0.10	0.30	0.29	m	0.30	0.30
Sample	1,633	1,271	1,133	5,744	5,276	5,639	3,856	5,663	4,589	1,638	1,225	1,255	5,635	5,061	5,480	3,095	5,321	4,283

(1) These samples exclude individuals with either missing data on either: wages (if employed); highest qualification; or employment experience. (2) Means for 1946 cohort samples are weighted to account for stratification. (3) The maths and reading scores are derived from tests taken at age 10 or 11. These scores are standardised for the sample of boys and girls who took the tests with mean 0 and standard deviation 1. A dummy variable is included if the test was not taken and scores are missing. (4) The qualification variables represent the level of the highest qualification attained at the given survey. Academic or vocational qualifications are counted. The variables taking the value 1 if the qualification level is the highest attained and 0 otherwise. (5) This is set to zero for non-employees. (6) The variable 'London or SE' is a also a dummy variable taking the value 1 for individuals living in London or the South East and zero for those living in other parts of Britain. This was missing for the postal survey of the 1970 cohort carried out at age 26.

Table 7.3: Variable means for cross-section samples of employees

	Women								
	1946 cohort			*1958 cohort*			*1970 cohort*		
	26	*31*	*43*	*23*	*33*	*42*	*26*	*30*	*34*
Maths score	+0.03	-0.03	0.00	+0.18	+0.12	+0.07	+0.18	+0.09	+0.11
Reading score	+0.02	-0.02	0.01	+0.17	+0.13	+0.08	+0.27	+0.17	+ 0.19
Missing score	0.09	0.10	0.10	0.13	0.14	0.14	0.23	0.24	0.23
O-level	0.27	0.27	0.26	0.39	0.38	0.31	0.41	0.29	0.25
A-level	0.12	0.08	0.10	0.15	0.10	0.13	0.11	0.15	0.16
Diploma	0.09	0.06	0.09	0.13	0.16	0.18	0.12	0.16	0.21
Degree	0.04	0.04	0.08	0.11	0.13	0.14	0.22	0.21	0.22
Years in current job	2.59	3.11	5.82	3.24	4.54	6.77	3.80	4.29	5.22
Years in work	0.88	4.58	13.35	5.30	11.58	18.15	5.93	10.26	13.79
London or SE	0.33	0.25	0.30	0.10	0.30	0.28	m	0.30	0.28
Sample	692	514	836	3,514	3,128	3,998	2,841	3,894	3,004

	Men								
	1946 cohort			*1958 cohort*			*1970 cohort*		
	26	*31*	*43*	*23*	*33*	*42*	*26*	*30*	*34*
Maths score	-0.09	-0.04	-0.02	+0.08	+0.19	+0.16	+0.28	+0.14	+0.18
Reading score	-0.12	-0.08	-0.04	+0.07	+0.16	+0.13	+0.19	+0.05	+0.08
Missing score	0.11	0.11	0.10	0.14	0.14	0.13	0.25	0.24	0.25
O-level	0.14	0.14	0.13	0.32	0.30	0.27	0.33	0.24	0.20
A-level	0.14	0.13	0.16	0.23	0.19	0.18	0.14	0.24	0.24
Diploma	0.12	0.13	0.12	0.10	0.16	0.18	0.10	0.14	0.16
Degree	0.10	0.10	0.16	0.09	0.16	0.18	0.24	0.20	0.23
Years in current job	2.97	4.58	10.1	3.51	6.52	10.66	4.06	5.27	7.17
Years in work	0.98	5.84	17.67	5.85	13.97	21.97	5.11	11.02	15.01
London or SE	0.31	0.29	0.30	0.10	0.30	0.29	m	0.30	0.30
Sample	1,385	974	902	4,263	3,672	3,957	2,375	4,166	3,289

Table 7.4: Variable means for cross-section samples of full-time employees

	Women									**Men**								
	1946 cohort			*1958 cohort*			*1970 cohort*			*1946 cohort*			*1958 cohort*			*1970 cohort*		
	26	*31*	*43*	*23*	*33*	*42*	*26*	*30*	*34*	*26*	*31*	*43*	*23*	*33*	*42*	*26*	*30*	*34*
Maths score	+0.12	+0.20	+0.11	+0.17	+0.25	+0.12	+0.22	+0.17	+0.17	-0.09	-0.04	-0.02	+0.06	+0.18	+0.17	+0.29	+0.15	+0.18
Reading score	+0.12	+0.14	+0.13	+0.15	+0.25	+0.13	+0.30	+0.24	+ 0.25	-0.12	-0.08	-0.05	+0.06	+0.16	+0.14	+0.19	+0.05	+0.09
Missing score	0.09	0.10	0.13	0.13	0.14	0.14	0.23	0.24	0.23	0.11	0.11	0.10	0.14	0.14	0.13	0.25	0.24	0.25
O-level	0.32	0.30	0.28	0.40	0.34	0.30	0.40	0.27	0.20	0.14	0.14	0.13	0.33	0.30	0.27	0.33	0.24	0.20
A-level	0.13	0.12	0.09	0.15	0.12	0.13	0.12	0.16	0.16	0.14	0.13	0.16	0.23	0.19	0.18	0.14	0.24	0.24
Diploma	0.11	0.09	0.13	0.13	0.19	0.20	0.12	0.18	0.23	0.12	0.13	0.12	0.09	0.19	0.18	0.10	0.14	0.16
Degree	0.05	0.08	0.10	0.10	0.18	0.17	0.24	0.26	0.29	0.10	0.10	0.16	0.08	0.16	0.18	0.24	0.20	0.23
Years in current job	3.10	4.05	6.62	3.61	5.81	8.01	3.95	5.02	6.39	2.98	4.58	10.07	3.73	6.54	10.82	4.10	5.30	7.24
Years in full-time work	0.95	4.85	12.42	5.46	11.85	16.22	5.60	9.97	13.01	0.98	5.83	17.61	5.94	13.94	21.94	5.00	10.90	14.87
Years in part-time work	-	0.29	2.14	5.39	0.68	2.79	0.25	0.42	1.00	-	0.00	0.05	0.01	0.07	0.12	0.11	0.15	0.17
London or SE	0.34	0.28	0.31	0.10	0.33	0.28	m	0.32	0.30	0.31	0.29	0.30	0.10	0.30	0.29	m	0.31	0.31
Sample	526	243	450	2,813	1,673	2,277	2,419	2,736	1,773	1,383	974	890	3,651	3,584	3,879	2,334	4,120	3,223

7.2.3 Results from log-linear wage models

Table 7.5 shows the partial linear correlations between log wages and selected characteristics, estimated by least squares for the cross-sections of women and men employees at each survey.

Men's predicted pay is systematically higher than women's for each of the three cohorts at each survey age. Most of this higher pay is captured in the higher constant term in the wage equations. The wage returns to employment experience are also higher for men than for women in the 1946 cohort at age 26 and the 1958 cohort at age 23.

Gender differences in the patterns of estimated coefficients suggest larger differences in pay between unqualified women and men than between qualified women and men i.e the coefficients on qualifications are larger in the female wage equations than in male wage equations. Amongst both unqualified and qualified employees though, women's predicted wage is lower than men's once the constant term in each wage equation is added.

The wage returns to holding qualifications above O-level increase with age for both women and men. This coincides with the increasing dispersion of wages with age for both genders (see figures 5.9 and 5.10 in the appendix to chapter 5).

Table 7.6 shows the OLS estimated wage models for full-time employees at each survey. The pattern of returns for for women working full-time is closer to that for men than it is to the pattern estimated for all working women (including part-time employees). The constant term in the female wage equation estimated for full-time employees is higher than that for all female employees. In turn, the relative returns to having qualifications above O-level tend to be lower for female full-time employees than for all female employees. Years in employment are separated into full-time and part-time experience This reveals a pattern of positive returns to full-time experience, but non-significant or negative returns to part-time experience.

Table 7.5: Least squares estimates of effects of education, employment experience and other characteristics on log wages

	Women									**Men**								
	1946 cohort			*1958 cohort*			*1970 cohort*			*1946 cohort*			*1958 cohort*			*1970 cohort*		
	26	*31*	*43*	*23*	*33*	*42*	*26*	*30*	*34*	*26*	*31*	*43*	*23*	*33*	*42*	*26*	*30*	*34*
Maths score at age 11	+0.03	+0.03	+0.05	+0.04	+0.03	+0.04	+0.02	+0.08	+0.07	+0.04	+0.06	+0.07	+0.02	+0.06	+0.07	+0.03	+0.07	+0.05
	(0.02)	(0.03)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Reading score at age 11	+0.05	+0.03	+0.07	+0.03	+0.02	+0.01	+0.05	+0.01	+0.03	+0.02	+0.02	+0.06	+0.01	+0.02	+0.04	+0.02	+0.03	+0.04
	(0.02)	(0.03)	(0.03)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Missing maths score	+0.02	+0.01	+0.08	-0.02	+0.04	+0.03	+0.03	+0.01	+0.01	-0.01	-0.02	+0.01	-0.01	0.00	-0.04	+0.01	-0.03	+0.01
	(0.04)	(0.04)	(0.08)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.05)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Highest qualification (no quals = ref):																		
O-level or equivalent	+0.08	+0.16	+0.13	+0.11	+0.09	+0.10	+0.13	+0.06	+0.02	+0.01	+0.07	+0.17	+0.10	+0.14	+0.10	+0.10	+0.06	+0.04
	(0.04)	(0.05)	(0.06)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)	(0.04)	(0.05)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)
A-level or equivalent	+0.17	+0.30	+0.19	+0.20	+0.26	+0.20	+0.18	+0.17	+0.11	+0.10	+0.15	+0.24	+0.16	+0.22	+0.21	+0.15	+0.16	+0.11
	(0.04)	(0.07)	(0.07)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)	(0.04)	(0.05)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)
Diploma	+0.58	+0.66	+0.64	+0.28	+0.49	+0.38	+0.30	+0.36	+0.32	+0.14	+0.19	+0.25	+0.20	+0.35	+0.30	+0.22	+0.29	+0.22
	(0.05)	(0.06)	(0.07)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)	(0.06)	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)
Degree or higher	+0.60	+0.66	+0.49	+0.39	+0.72	+0.63	+0.39	+0.52	+0.57	+0.27	+0.36	+0.40	+0.31	+0.53	+0.50	+0.32	+0.46	+0.48
	(0.06)	(0.06)	(0.08)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.05)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Years in current job	+0.01	+0.02	+0.02	+0.02	+0.02	+0.01	+0.01	+0.01	+0.01	0.00	-0.01	0.00	+0.01	+0.01	+0.01	+0.02	+0.01	+0.01
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Years in work (1)	+0.01	+0.02	+0.03	+0.01	+0.03	+0.02	0.00	+0.02	+0.02	+0.04	+0.02	+0.04	+0.04	+0.02	+0.01	0.00	+0.02	+0.01
	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.02)	(0.02)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
London or SE	+0.22	+0.05	+0.21	-0.04	+0.18	+0.11	-	+0.17	+0.19	+0.09	+0.14	+0.18	-0.01	+0.22	+0.21	-	+0.23	+0.27
	(0.03)	(0.04)	(0.05)	(0.02)	(0.02)	(0.02)		(0.01)	(0.02)	(0.02)	(0.02)	(0.04)	(0.02)	(0.01)	(0.02)		(0.02)	(0.02)
Constant	1.07	1.14	0.98	1.29	1.15	1.26	1.64	1.41	1.42	1.28	1.64	1.19	1.35	1.61	1.69	1.75	1.63	1.76
	(0.05)	(0.04)	(0.09)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)	(0.11)	(0.13)	(0.31)	(0.03)	(0.04)	(0.06)	(0.03)	(0.04)	(0.05)
Sample	692	514	836	3,514	3,128	3,998	2,841	3,894	3,004	1,385	974	902	4,263	3,672	3,957	2,375	4,166	3,289
Adjusted R^2	0.29	0.30	0.50	0.31	0.36	0.43	0.42	0.43	0.46	0.19	0.28	0.42	0.34	0.36	0.47	0.44	0.43	0.46

Notes: The table reports coefficients estimated by least-squares fitting from 18 log-linear wage models. Standard errors are shown in parentheses.
- indicates that variables are missing. d indicates that a variable was dropped from the model owing to collinearity.
(1) For the age twenty-six sample of the 1946 cohort only, work experience is measured in months over the last year (since age twenty-five), rather than in years.

Table 7.6: Least squares estimates of effects of education, employment experience and other characteristics on full-time log wages

	Women									**Men**								
	1946 cohort			*1958 cohort*			*1970 cohort*			*1946 cohort*			*1958 cohort*			*1970 cohort*		
	26	*31*	*43*	*23*	*33*	*42*	*26*	*30*	*34*	*26*	*31*	*43*	*23*	*33*	*42*	*26*	*30*	*34*
Maths score at age 11	+0.06	+0.06	+0.08	+0.05	+0.05	+0.06	+0.03	+0.07	+0.07	+0.04	+0.06	+0.07	+0.02	+0.06	+0.06	+0.03	+0.06	+0.05
	(0.02)	(0.04)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Reading score at age 11	+0.01	-0.01	0.00	+0.02	+0.03	+0.01	+0.03	+0.02	+0.02	+0.02	+0.02	+0.06	+0.02	+0.02	+0.04	+0.02	+0.03	+0.04
	(0.02)	(0.04)	(0.03)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Missing maths score	+0.01	-0.03	+0.02	-0.02	+0.07	+0.04	+0.01	+0.02	+0.03	-0.01	-0.02	+0.01	+0.02	0.00	-0.03	+0.01	-0.03	+0.01
	(0.04)	(0.07)	(0.07)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.06)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Highest qualification (no quals = ref):																		
O-level or equivalent	+0.11	+0.16	+0.26	+0.08	+0.11	+0.12	+0.11	+0.07	-0.03	+0.01	+0.07	+0.18	+0.08	+0.14	+0.10	+0.10	+0.06	+0.04
	(0.04)	(0.05)	(0.06)	(0.01)	(0.02)	(0.02)	(0.03)	(0.02)	(0.04)	(0.03)	(0.04)	(0.05)	(0.01)	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)
A-level or equivalent	+0.20	+0.25	+0.16	+0.15	+0.26	+0.23	+0.17	+0.16	+0.04	+0.10	+0.15	+0.25	+0.15	+0.22	+0.21	+0.15	+0.16	+0.11
	(0.04)	(0.07)	(0.10)	(0.02)	(0.03)	(0.03)	(0.03)	(0.02)	(0.04)	(0.03)	(0.04)	(0.05)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)
Diploma	+0.54	+0.51	+0.62	+0.21	+0.41	+0.37	+0.27	+0.29	+0.22	+0.14	+0.19	+0.25	+0.18	+0.35	+0.29	+0.22	+0.29	+0.22
	(0.05)	(0.06)	(0.07)	(0.02)	(0.03)	(0.03)	(0.04)	(0.02)	(0.04)	(0.03)	(0.03)	(0.06)	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)
Degree or higher	+0.57	+0.55	+0.58	+0.33	+0.59	+0.57	+0.36	+0.43	+0.41	+0.27	+0.36	+0.41	+0.28	+0.53	+0.49	+0.32	+0.46	+0.48
	(0.06)	(0.06)	(0.08)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.04)	(0.05)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Years in current job	+0.01	+0.00	0.00	+0.02	+0.01	+0.01	+0.01	+0.01	0.00	0.00	-0.01	0.00	+0.01	+0.00	+0.01	+0.01	+0.01	+0.01
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Full-time exp (1)	+0.01	+0.03	+0.03	0.00	+0.02	+0.02	+0.00	+0.01	+0.02	+0.04	+0.02	+0.05	+0.04	+0.02	+0.01	0.00	+0.02	+0.01
	(0.01)	(0.02)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.02)	(0.02)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Part-time exp (1)	d	+0.02	+0.02	-0.05	-0.01	+0.00	-0.02	-0.01	-0.01	d	-0.01	-0.01	-0.05	-0.01	+0.01	-0.01	-0.02	-0.03
		(0.02)	(0.01)	(0.02)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)		(0.03)	(0.03)	(0.03)	(0.01)	(0.00)	(0.00)	(0.00)	(0.01)
London or SE	+0.23	+0.06	+0.22	-0.05	+0.18	+0.15	-	+0.18	+0.20	+0.09	+0.14	+0.17	-0.03	+0.21	+0.21	-	+0.24	+0.27
	(0.03)	(0.05)	(0.04)	(0.02)	(0.02)	(0.02)		(0.01)	(0.02)	(0.02)	(0.02)	(0.04)	(0.02)	(0.01)	(0.02)		(0.02)	(0.02)
Constant	1.18	1.28	1.07	1.36	1.38	1.39	1.69	1.59	1.66	1.28	1.64	1.15	1.36	1.62	1.76	1.77	1.63	1.78
	(0.08)	(0.08)	(0.10)	(0.03)	(0.04)	(0.04)	(0.04)	(0.05)	(0.06)	(0.11)	(0.13)	(0.33)	(0.03)	(0.04)	(0.06)	(0.03)	(0.04)	(0.05)
Sample	526	243	450	2,813	1,673	2,277	2,419	2,736	1,773	1,383	974	890	3,651	3,584	3,879	2,334	4,120	3,223
Adjusted R^2	0.26	0.29	0.39	0.27	0.33	0.43	0.40	0.43	0.46	0.29	0.28	0.42	0.30	0.36	0.47	0.44	0.43	0.46

(1) For the age twenty-six sample of the 1946 cohort only, work experience is measured in months over the last year (since age twenty-five), rather than in years.

7.2.4 Results from probit models for propensity-score matching

All employees

The probit models summarised in table 7.7 show the differences in the characteristics of women and men employees at each survey. The negative coefficients on years in work show that women are likely to have less employment experience than men at each survey. The other coefficients should be interpreted with some caution. For example, for the 1970 cohort at age 34, the raw difference in the proportion of female and male employees with a degree level qualification is negligible, but the estimated probit parameter is negative and significant owing to the strong positive association between labour market attachment and education for women. The pseudo R^2 for each model gives a crude indicator of the extent to which women and men employees differ across the selected characteristics. This shows that women and men become more different with age, owing to the widening gender gap in employment experience.

Table 7.8 shows the standardised differences in the means of each covariate across the unmatched and matched samples. This provides an indication of how well propensity-score matching balances covariates across the matched sample. If there were no systematic differences after matching, we would expect to see standardised differences (t-statistics) of less than 1.96. Where we see standardised differences larger than this across the matched samples, it is a sign that certain covariates are not balanced by the matching exercise. Only individuals with a propensity-score outside the range estimated for the opposite gender are excluded from the analysis i.e. common support restrictions are imposed.

We find that the weighted sample of male employees that is matched to female employees is systematically less likely to have a degree for the two later cohorts. The interpretation of this is that men with low levels of employment experience relative to other men, but close to levels held by female employees, tend to be less qualified. This effect also comes through in lower reading and maths scores in some of the samples. The consequence of this is that the estimate of unequal treatment may be biased downward, since we are comparing the pay of women to that of less qualified men.

We find that the weighted sample of female employees that is matched to male employees (the second matched sample) is systematically more likely to have a degree in four out of the nine samples and to have higher maths and reading scores in two of the 1946 cohort samples. The consequence of this is again that, in these cases, the estimate of unequal treatment is likely to be biased downward.

Table 7.7: Estimated probit regression parameters from models used to estimate propensity-scores, cross-section samples of employees

	1946 cohort			*1958 cohort*			*1970 cohort*		
Age at survey	*26*	*31*	*43*	*23*	*33*	*42*	*26*	*30*	*34*
Father in non-manual job (wt.)	+0.03 (0.06)	0.00 (0.08)	0.00 (0.08)	-	-	-	-	-	-
Maths score at age 11	+0.12 (0.05)	+0.13 (0.06)	+0.06 (0.06)	+0.02 (0.02)	0.00 (0.03)	-0.03 (0.02)	-0.30 (0.03)	-0.23 (0.02)	-0.23 (0.03)
Reading score at age 11	+0.05 (0.05)	+0.04 (0.06)	+0.11 (0.06)	+0.03 (0.02)	+0.06 (0.03)	+0.06 (0.02)	+0.29 (0.03)	+0.28 (0.02)	+0.27 (0.03)
(missing maths score)	-0.08 (0.09)	+0.07 (0.12)	+0.18 (0.12)	-0.07 (0.04)	-0.03 (0.05)	-0.06 (0.04)	-0.06 (0.04)	0.00 (0.03)	-0.08 (0.04)
Highest qualification (no quals = ref):									
O-level or equivalent	+0.35 (0.09)	+0.25 (0.11)	+0.33 (0.11)	+0.19 (0.04)	+0.15 (0.05)	+0.15 (0.05)	+0.34 (0.05)	+0.13 (0.04)	+0.24 (0.05)
A-level or equivalent	-0.03 (0.10)	-0.19 (0.12)	-0.33 (0.13)	-0.47 (0.05)	-0.48 (0.06)	-0.16 (0.05)	+0.09 (0.07)	-0.30 (0.05)	-0.16 (0.05)
Diploma	-0.01 (0.10)	-0.50 (0.14)	-0.12 (0.13)	0.00 (0.06)	-0.06 (0.06)	+0.08 (0.05)	+0.32 (0.07)	0.00 (0.05)	+0.18 (0.06)
Degree or higher	-0.65 (0.12)	-0.73 (0.15)	-0.41 (0.13)	-0.75 (0.08)	-0.82 (0.07)	-0.43 (0.06)	+0.28 (0.07)	-0.34 (0.06)	-0.22 (0.06)
Years in work	-1.71 (0.18)	-0.48 (0.04)	-0.41 (0.02)	-0.19 (0.01)	-0.16 (0.01)	-0.10 (0.00)	+0.04 (0.01)	-0.06 (0.01)	-0.06 (0.01)
Years in current job	-0.01 (0.01)	-0.04 (0.01)	-0.01 (0.01)	+0.02 (0.05)	0.00 (0.00)	-0.02 (0.00)	-0.01 (0.01)	-0.02 (0.00)	-0.03 (0.00)
Living in London/SE	+0.08 (0.06)	-0.05 (0.08)	-0.01 (0.08)	+0.01 (0.05)	+0.03 (0.04)	-0.03 (0.03)	-	-0.03 (0.03)	-0.08 (0.04)
Constant term	+1.23 (0.19)	+2.48 (0.23)	+6.69 (0.39)	+0.98 (0.08)	+2.16 (0.08)	+2.30 (0.08)	-0.30 (0.07)	+0.85 (0.08)	+1.09 (0.09)
Pseudo R^2	0.07	0.19	0.38	0.04	0.13	0.13	0.03	0.04	0.06
Sample size	2,077	1,488	1,738	7,777	6,800	7,955	5,216	8,060	6,293
N (women)	692	514	836	3,514	3,128	3,998	2,841	3,894	3,004
N (men)	1,385	974	902	4,263	3,672	3,957	2,375	4,166	3,289

The coefficients shown are estimated probit regression parameters (not marginal effects) from nine separate models that discriminate between employed samples of women and men based on selected characteristics included in the models. A positive coefficient indicates the greater prevalence of the associated characteristic in the female vs. the male sample. Standard errors are given in brackets.
- indicates that variables are missing. d indicates that a variable was dropped from the model owing to collinearity.

Table 7.8: Standardised gender differences in means of covariates (t-values) before and after matching, all employees

		1946 cohort			*1958 cohort*			*1970 cohort*		
Age at survey		*26*	*31*	*43*	*23*	*33*	*42*	*26*	*30*	*34*
Father in non-manual job (wt.)	Unmatched (1)	+0.39	-1.33	-0.18	-	-	-	-	-	-
	Matched (1)	0.00	+0.82	-8.63	-	-	-	-	-	-
	Matched (2)	+3.05	+1.71	+3.53	-	-	-	-	-	-
Maths score at age 11	Unmatched (1)	+3.22	+0.31	+0.44	+4.97	-2.97	-4.33	-4.51	-2.79	-3.18
	Matched (1)	-0.05	+0.90	-4.12	-2.37	-1.74	-1.05	+0.04	-1.90	+0.60
	Matched (2)	+3.11	+2.32	+0.90	+0.68	+0.97	+0.36	-1.42	-0.03	-1.08
Reading score at age 11	Unmatched (1)	+3.27	+1.13	+1.62	+5.04	-1.59	-2.86	+3.84	+6.35	+5.31
	Matched (1)	-0.41	-0.18	-3.22	-2.95	-1.85	-2.17	+0.54	-2.27	+0.53
	Matched (2)	+2.95	+2.53	-1.39	+0.72	+1.65	-0.08	-0.36	+0.32	-0.79
(missing maths score)	Unmatched (1)	-0.67	-0.18	+0.31	-1.01	+0.44	+0.34	-1.11	+0.29	-1.42
	Matched (1)	-0.26	+1.03	+0.82	+0.11	+0.77	-1.31	-1.06	+0.72	-0.97
	Matched (2)	+0.12	-0.14	-0.88	+1.49	+2.51	+2.28	+3.18	+1.17	+0.77
O-level or equivalent	Unmatched (1)	+6.47	+6.56	+7.24	+6.71	+7.08	+4.09	+6.31	+5.90	+5.09
	Matched (1)	-0.19	-2.63	+2.77	+1.79	+1.49	+2.76	+0.65	+1.60	+0.96
	Matched (2)	0.00	-1.33	-1.43	0.00	-2.17	-3.46	-0.03	-1.83	-0.65
A-level or equivalent	Unmatched (1)	+0.77	-1.23	-2.27	-9.02	-10.75	-5.64	-3.11	-9.43	-7.82
	Matched (1)	0.00	+1.07	+1.99	+0.68	+0.39	+1.13	+0.21	+1.83	+1.15
	Matched (2)	-0.05	+0.31	+2.31	+2.16	+1.41	-1.55	-0.33	-0.13	-1.29
Diploma	Unmatched (1)	+0.38	-3.33	-0.79	+5.10	+0.02	-0.16	+1.81	+2.68	+4.52
	Matched (1)	-0.08	0.00	-10.83	+2.51	-1.88	-1.58	-0.90	-0.46	-0.13
	Matched (2)	-0.59	+2.99	+1.42	+0.37	-0.83	+1.27	-0.58	+0.90	-0.13

Table 7.8: Standardised gender differences in means of covariates (t-values) before and after matching - continued

		1946 cohort			*1958 cohort*			*1970 cohort*		
Age at survey		*26*	*31*	*43*	*23*	*33*	*42*	*26*	*30*	*34*
Degree or higher	Unmatched (1)	-5.31	-5.12	-5.81	+2.73	-3.64	-4.34	-2.13	+0.43	-0.55
	Matched (1)	0.62	-1.69	-0.24	-2.78	-2.34	-2.98	-0.45	-1.85	-2.23
	Matched (2)	+2.54	+0.31	-0.06	-0.38	+3.40	+4.24	+0.17	+2.43	+1.11
Years in work	Unmatched (1)	-10.62	-18.56	-31.78	-12.12	-29.86	-36.05	+8.59	-11.42	-13.87
	Matched (1)	+0.16	+0.03	+3.14	+1.36	+1.05	+0.36	-0.44	+0.26	+1.35
	Matched (2)	+0.22	-1.97	-1.65	-1.19	-3.33	-2.91	-0.96	-1.87	-0.62
Years in current job	Unmatched (1)	-2.93	-6.99	-12.23	-4.75	-15.40	-23.12	-2.96	-10.47	-15.01
	Matched (1)	+0.40	+0.66	+5.37	-1.64	+0.22	+2.19	+1.15	-0.47	+1.35
	Matched (2)	-1.87	+0.86	+0.48	-1.56	+5.03	+2.09	-0.55	-0.19	+0.46
Living in London/SE	Unmatched (1)	+1.39	-1.48	+0.37	-0.01	-0.57	-0.54	-	-0.28	-1.87
	Matched (1)	+0.80	-0.21	+1.17	+0.57	+0.44	+0.55	-	-0.02	1.86
	Matched (2)	+0.81	+2.24	+0.96	-0.41	-0.94	-1.20	-	+0.59	-1.65

The statistics shown are the t-statistics (the difference in means of covariates divided by the standard error of this difference), based on tests for the equality of means of each covariate in female and male employees, before and after matching. A positive difference signals a greater relative prevalence or quantity of a characteristic amongst female compared to male employees. A difference with an t-value of 1.96 or higher (in magnitude) is interpreted as a statistically significant difference at the 95% significance level. (1) The treatment group is female employees in this model. The differences reflect the distribution of characteristics across female employees compared to all male employees for the unmatched samples and compared to a weighted sample of male employees for the matched samples. (2) The treatment group is male employees in this model. The differences reflect the distribution of characteristics across male employees compared to a weighted sample of female employees.

Full-time employees

The probit models summarised in table summarised in 7.9 show the divergence in women's and men's employment patterns for full-time employees - with women having spent less time in full-time work and more time in part-time work than men at each survey. The summary of differences in covariates (table 7.10) shows that systematic differences in levels of full-time and part-time experience remain across the matched samples of female and male full-time employees. Consequently, the estimates of unequal treatment from these models may be biased upward, since differences may in part be attributable to these differences in employment experience.

Table 7.9: Estimated probit regression parameters from models used to estimate propensity-scores, cross-section samples of full-time employees

	1946 cohort			1958 cohort			1970 cohort		
Age at survey	26	31	43	23	33	42	26	30	34
Father in non-manual job (wt.)	+0.10 (0.07)	+0.08 (0.09)	-0.07 (0.09)	-	-	-	-	-	-
Maths score at age 11	+0.09 (0.05)	+0.13 (0.07)	+0.13 (0.07)	+0.02 (0.03)	-0.01 (0.03)	-0.02 (0.03)	-0.30 (0.03)	-0.22 (0.03)	-0.22 (0.03)
Reading score at age 11	+0.09 (0.05)	+0.04 (0.07)	+0.12 (0.07)	+0.03 (0.03)	+0.09 (0.03)	+0.04 (0.03)	+0.29 (0.03)	+0.29 (0.03)	+0.26 (0.03)
(missing maths score)	-0.05 (0.10)	+0.10 (0.13)	+0.26 (0.14)	-0.08 (0.05)	+0.02 (0.05)	+0.01 (0.05)	-0.07 (0.04)	0.00 (0.04)	-0.08 (0.04)
Highest qualification (no quals = ref):									
O-level or equivalent	+0.49 (0.09)	+0.43 (0.13)	+0.43 (0.14)	+0.17 (0.04)	+0.16 (0.06)	+0.13 (0.06)	+0.41 (0.06)	+0.18 (0.05)	+0.21 (0.07)
A-level or equivalent	+0.08 (0.10)	+0.09 (0.15)	-0.17 (0.15)	-0.44 (0.06)	-0.24 (0.07)	-0.02 (0.07)	+0.21 (0.07)	-0.14 (0.05)	-0.03 (0.07)
Diploma	+0.17 (0.11)	-0.12 (0.16)	+0.20 (0.15)	+0.01 (0.06)	+0.10 (0.07)	+0.20 (0.06)	+0.48 (0.07)	+0.20 (0.06)	+0.36 (0.07)
Degree or higher	-0.42 (0.13)	-0.39 (0.17)	-0.22 (0.15)	-0.65 (0.09)	-0.31 (0.08)	-0.08 (0.07)	+0.05 (0.01)	+0.02 (0.06)	+0.16 (0.07)
Years in full-time work	-1.71 (0.18)	-0.35 (0.17)	-0.32 (0.03)	-0.17 (0.01)	-0.11 (0.01)	-0.08 (0.00)	+0.17 (0.03)	-0.05 (0.01)	-0.05 (0.01)
Years in part-time work	d	+0.20 (0.13)	+0.05 (0.05)	+0.27 (0.11)	+0.18 (0.02)	+0.25 (0.01)	+0.17 (0.03)	+0.12 (0.02)	+0.20 (0.02)
Years in current job	-0.08 (0.02)	0.00 (0.01)	+0.01 (0.01)	+0.03 (0.01)	+0.02 (0.00)	+0.01 (0.00)	+0.00 (0.01)	+0.01 (0.00)	0.00 (0.04)
Living in London/SE	+0.09 (0.07)	-0.02 (0.09)	-0.05 (0.10)	+0.01 (0.05)	+0.09 (0.04)	-0.03 (0.04)	-	+0.02 (0.03)	0.00 (0.04)
Constant term	+0.11 (0.24)	+0.98 (0.26)	+4.64 (0.43)	+0.77 (0.09)	+0.77 (0.11)	+0.95 (0.10)	-0.58 (0.08)	+0.12 (0.09)	+0.09 (0.11)
Pseudo R^2	0.05	0.12	0.39	0.03	0.09	0.25	0.04	0.04	0.09
Sample size	1,909	1,217	1,340	6,975	5,329	6,157	4,753	6,856	5,039
N (women)	526	243	450	3,050	1,691	2,278	2,419	2,736	1,787
N (men)	1,383	974	890	3,925	3,638	3,879	2,334	4,120	3,244

Table 7.10: Standardised gender differences in means of covariates (t-values) before and after matching, full-time employees

		1946 cohort			*1958 cohort*			*1970 cohort*		
Age at survey		*26*	*31*	*43*	*23*	*33*	*42*	*26*	*30*	*34*
Father in non-manual job (wt.)	Unmatched (1)	+1.85	+0.56	-0.61	-	-	-	-	-	-
	Matched (1)	+0.25	+0.37	-1.23	-	-	-	-	-	-
	Matched (2)	+0.54	-2.28	+3.73	-	-	-	-	-	-
Maths score at age 11	Unmatched (1)	+4.53	+2.86	+2.14	+4.39	+2.29	-2.10	-3.12	+0.92	-0.29
	Matched (1)	-1.16	+0.09	+1.92	-0.95	-2.47	+3.97	+1.70	-0.72	-0.16
	Matched (2)	-0.09	+0.25	-0.48	+1.95	+0.33	-0.65	-0.81	+0.66	-0.54
Reading score at age 11	Unmatched (1)	+4.71	+2.54	+2.99	+4.33	+3.55	-0.60	+4.80	+9.10	+6.75
	Matched (1)	-0.21	+0.17	+0.89	-1.55	-2.60	+2.84	+1.22	-1.84	-1.26
	Matched (2)	-0.46	+1.40	-0.77	+2.32	-2.16	-0.03	-0.11	+0.49	-1.51
(missing maths score)	Unmatched (1)	-0.33	+0.50	0.00	-1.20	+0.33	+1.17	-1.27	+0.30	-1.03
	Matched (1)	-0.29	+0.14	-1.85	-1.71	-0.25	-3.65	+0.41	-0.91	-1.25
	Matched (2)	+3.16	-1.71	-0.47	+0.46	-1.04	+2.41	+0.91	-1.46	+0.32
O-level or equivalent	Unmatched (1)	+7.23	+5.77	+6.70	+6.32	+2.91	+2.17	+5.08	+1.99	-0.17
	Matched (1)	+0.35	-0.10	+4.12	-0.99	-1.44	+4.08	-2.19	+0.40	+0.34
	Matched (2)	-0.17	-0.99	-2.51	-0.39	-0.91	-1.26	-0.94	+0.49	+1.11
A-level or equivalent	Unmatched (1)	+0.97	+0.25	-2.89	-8.38	-6.15	-4.78	-2.67	-8.06	-6.69
	Matched (1)	-0.16	-0.73	-0.72	+2.48	+0.80	-0.35	+0.49	+0.94	+1.90
	Matched (2)	+0.47	-2.92	-0.19	+3.40	+0.71	-0.30	-0.08	+1.82	-1.03
Diploma	Unmatched (1)	+2.01	-0.87	+0.88	+4.42	+2.16	+1.94	+2.49	+4.30	+5.72
	Matched (1)	-0.75	+1.52	-1.01	+0.95	+1.49	-3.03	-0.82	-0.42	-0.91
	Matched (2)	-1.25	+1.69	-0.79	+0.52	-1.52	-0.15	-0.24	-1.47	-0.13

Table 7.10: Standardised gender differences in means of covariates (t-values) before and after matching - continued

		1946 cohort			*1958 cohort*			*1970 cohort*		
Age at survey		*26*	*31*	*43*	*23*	*33*	*42*	*26*	*30*	*34*
Degree or higher	Unmatched (1)	-3.79	-2.23	-3.72	+2.23	+2.10	-0.73	-0.11	+5.67	+4.95
	Matched (1)	-0.97	-0.75	-0.21	-0.96	-1.91	-1.54	+2.48	-0.64	-1.13
	Matched (2)	-0.05	+1.48	-0.42	-1.66	-0.10	-1.48	+0.61	-0.58	+0.06
Years in full-time work	Unmatched (1)	-3.28	-11.95	-29.16	-9.38	-21.29	-42.88	+5.84	-12.19	-17.16
	Matched (1)	+0.17	+0.34	+1.75	-0.86	+1.23	+5.02	-1.37	-0.29	+1.65
	Matched (2)	-2.02	-2.13	-1.43	-1.32	-0.84	+2.53	-0.91	+0.42	+1.76
Years in part-time work	Unmatched (1)	d	+6.98	+18.75	+4.68	+19.27	+41.66	+5.97	+10.71	+18.65
	Matched (1)	d	+1.79	-1.51	+1.93	+2.81	+2.48	+2.89	+2.21	+1.81
	Matched (2)	d	+2.32	+2.25	+1.40	+4.77	+3.77	+1.01	+2.67	+2.64
Years in current job	Unmatched (1)	-0.15	-1.99	-7.92	-2.47	-4.57	-13.25	-1.56	-2.65	-5.45
	Matched (1)	+0.53	-0.18	+2.11	-1.51	-2.89	-0.05	-0.63	-0.78	-0.71
	Matched (2)	-2.23	+0.03	+0.78	-2.69	-3.35	+1.09	+0.18	-0.23	+0.99
Living in London/SE	Unmatched (1)	+1.79	-0.69	+0.85	-0.02	+1.93	-0.94	-	+1.26	-0.07
	Matched (1)	-0.83	0.00	+0.36	+0.09	-0.87	-2.25	-	-1.44	-1.12
	Matched (2)	-1.15	+2.15	+0.46	+0.19	-1.62	+1.79	-	+0.14	+1.87

(1) The treatment group is female full-time employees in this model. A positive difference signals a greater relative prevalence or quantity of a characteristic amongst female compared to unmatched or matched samples of male full-time employees.
(2) The treatment group is male full-time employees in this model. A positive difference signals a greater relative prevalence of a characteristic amongst (matched) female compared to male full-time employees.

7.2.5 Estimates of unequal treatment

All employees

The different estimators are summarised in table 7.11. Table 7.12 shows the set of estimates. All estimates are positive and statistically different from zero, suggesting that unequal treatment of women and men has impacts on pay for all three birth cohorts. Systematic patterns of cross and within-cohort differences are also revealed:

1. The unexplained gap is higher for the 1946 cohort at ages twenty-six and thirty-one than for the two later cohorts in their twenties and thirties.
2. The unexplained gap is higher for the 1958 cohort at ages twenty-three and thirty-three than for the 1970 cohort at ages twenty-six, thirty and thirty-four.
3. The unexplained gap is not higher for the 1946 cohort at age forty-three (in 1989) than for the 1958 cohort at age forty-two (in 2000).
4. The unexplained gap increases with age for the 1958 and 1970 cohorts.

Table 7.11: Summary of different estimators of unexplained log differential

	Method	*Estimator*
(1)	Decomposition (weights = male employees)	$\bar{X}'_{m,s=1}(\hat{\beta}_m - \hat{\beta}_f)$
(2)	Decomposition (weights = female employees)	$\bar{X}'_{f,s=1}(\hat{\beta}_m - \hat{\beta}_f)$
(3)	Decomposition (weights = all employees)	$\bar{X}_{m,s=1}(\hat{\beta}_m - \hat{\beta}^*) + \bar{X}_{f,s=1}(\hat{\beta}^* - \hat{\beta}_f)$
(4)	Decomposition (weights = all women)	$\bar{X}'_f(\hat{\beta}_m - \hat{\beta}_f)$
(5)	Propensity-score matching (weights = male employees)	$\frac{1}{N_m}\sum_{i \in m,s=1} \ln(w_{mi}) - \frac{1}{N_m}\sum_{i \in m,s=1} \ln(\hat{w}_{mi})$
(6)	Propensity-score matching (weights = female employees)	$\frac{1}{N_f}\sum_{i \in f,s=1} \ln(\hat{w}_{fi}) - \frac{1}{N_f}\sum_{i \in f,s=1} \ln(w_{fi})$

Notes: In (3) $\hat{\beta}^*$ is the vector of estimated parameters from a pooled model including a dummy for gender. In (5) $\ln(\hat{w}_{mi})$ is the counterfactual (female) estimated wage for male individual, i. In (6) $\ln(\hat{w}_{fi})$ is the counterfactual (male) estimated wage for female individual, i.

Looking first at cross-cohort trends, there is evidence of a decrease in unequal treatment at younger ages. For the 1946 cohort, the estimates of the unexplained gap for women and men at age twenty-six and thirty-one range from 0.23 log points to 0.43 log points. For the 1958 cohort at ages twenty-three and thirty-three, the range is 0.18-0.26 log points, and for the 1970 cohort at ages twenty-six, thirty and thirty-four, the range

Table 7.12: Estimates of unexplained log difference in pay for women and men employees using different estimators, by survey

	1946 cohort			*1958 cohort*			*1970 cohort*		
Age at survey	*26*	*31*	*43*	*23*	*33*	*42*	*26*	*30*	*34*
(1)	0.33 (0.02)	0.31 (0.02)	0.33 (0.04)	0.18 (0.01)	0.21 (0.01)	0.26 (0.01)	0.10 (0.01)	0.14 (0.01)	0.17 (0.01)
(2)	0.33 (0.02)	0.39 (0.04)	0.32 (0.08)	0.15 (0.01)	0.27 (0.01)	0.30 (0.01)	0.10 (0.01)	0.14 (0.01)	0.18 (0.01)
(3)	0.34 (0.02)	0.36 (0.02)	0.34 (0.03)	0.16 (0.01)	0.23 (0.01)	0.28 (0.01)	0.10 (0.01)	0.14 (0.01)	0.18 (0.01)
(4)	0.24 (0.05)	0.43 (0.06)	0.32 (0.09)	0.16 (0.01)	0.31 (0.01)	0.32 (0.01)	0.10 (0.01)	0.16 (0.01)	0.21 (0.01)
(5)	0.28 (0.03)	0.23 (0.04)	0.28 (0.06)	0.17 (0.01)	0.17 (0.02)	0.26 (0.02)	0.11 (0.02)	0.13 (0.02)	0.19 (0.02)
(6)	0.32 (0.03)	0.39 (0.04)	0.36 (0.13)	0.16 (0.01)	0.26 (0.02)	0.32 (0.02)	0.09 (0.02)	0.14 (0.02)	0.19 (0.02)
Raw log gap	0.38	0.44	0.53	0.17	0.35	0.40	0.11	0.17	0.22
N (women)	692	514	836	3,514	3,128	3,998	2,841	3,894	3,004
N (men)	1,385	974	902	4,263	3,672	3,957	2,375	4,166	3,289

Notes: see table 7.11 for description of each of the 6 estimators. Standard errors are reported in parentheses.

is 0.09-0.21. Comparing within estimator, the estimates are in each case higher for the 1946 cohort than for the two later cohorts, and higher for the 1958 cohort than for the 1970 cohort. In contrast, there is no evidence of a decrease in the unexplained gap across the 1946 cohort at age forty-two (in 1989) and the 1958 cohort at age forty-three (in 2000).

Looking at within-cohort trends, there is evidence of an increase in unequal treatment with age in the two more recent cohorts.[6] This is clear to see in figure 7.5, which graphs the estimates shown in table 7.12 for the 1958 and 1970 cohorts. Within the 1958 cohort, there is a marked increase in the unexplained log gap between the ages of twenty-three and forty-two. The timing and extent of this increase appears to vary across different groups of women and men, as evidenced by the variation in patterns across the different estimators:

- Using female employees as the reference sample (estimators (2) and (6)) suggests

[6]The standard errors associated with the 1946 cohort estimates are to wide, and the variation in estimates across estimators is too great, to get an informative picture of within-cohort change.

that the unexplained gap in pay increases early on for women who have weaker labour market attachment and lower levels of education.

- Using men as reference sample (estimators (1) and (5)) suggests that the unexplained gap in increases less and later on for women with stronger labour market attachment i.e. those who share average characteristics with men.

Within the 1970 cohort, the unexplained gap increased from around 0.1 log points at age twenty-six to around 0.2 log points by age thirty-four. In contrast to the 1958 cohort, the pattern of increase is consistent across estimators.

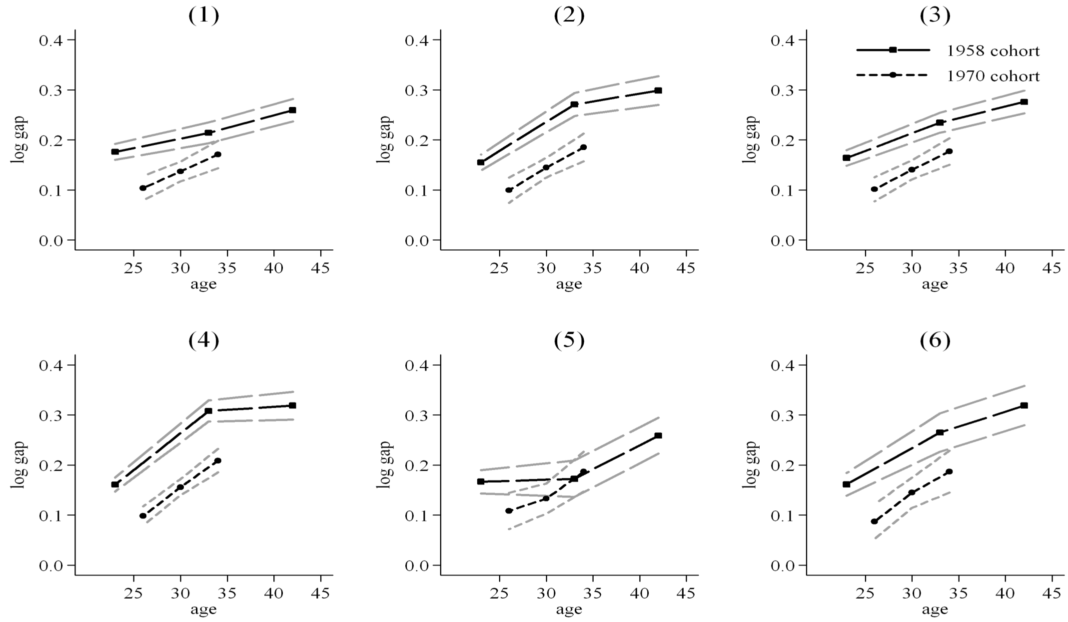

Figure 7.4: Estimates of unexplained log pay gap for the 1958 and 1970 cohort, by method.
The lighter, dashed lines show the 95% confidence intervals.

Investigation into the variation in the estimates across estimators revealed two main sources of sensitivity. A first source of variation comes from different weightings of returns to employment experience. For example:

- Using estimator (4), which weights the coefficients by the mean characteristics of the whole female population, rather than by those of employees, gender differences in returns to employment experience are less heavily weighted. The point estimate for the 1946 cohort at age twenty-six is smaller (0.24) using estimator (4), since the male advantage in returns to experience is weighted less in the estimate. However, the 95% confidence interval is wide (0.14, 0.34), reflecting the uncertainty with which the return to experiences are estimated for the 1946 cohort.[7]

- Using estimator (1) or (5), which uses the mean characteristics of male employees as the reference group, gender differences in returns to experience are more heavily weighted. For the 1958 cohort at age thirty-three, the unexplained log gap is smaller using estimators (1) or (5), giving more weight to the higher (relative) female return to work experience.

One interpretation of this pattern is that women with stronger labour market are less unequally paid, relative to their male counterparts, than women with weaker labour market attachment. However, table 7.8 revealed that women who have *equal* levels of employment experience to men are likely to be *more* highly qualified (more likely to have a degree) than their matched male counterparts. There is evidence then that the estimate of unequal treatment based on matching is downward biased for this group.

The second source of sensitivity is the weighting of job tenure. For the 1946 cohort at age thirty-one and the 1958 cohort at age thirty-three, using estimator (4) places a lower weight on the female advantage in returns to job tenure. Non-employed women (who feature in the reference sample) have been assigned a zero for job tenure. Since the model parameters were estimated for employees with varying levels of job tenure, the appropriateness of these weights, and the validity of resulting estimates, is questionable.

Full-time employees

Turning to the estimates for the restricted sample of full-time employees (table 7.13), we again see a clear cross-cohort decrease in estimates of unequal treatment. The estimates are also smaller than those for all employees (including part-time employees) at older ages. This suggests that higher levels of estimated unequal treatment in the thirties and forties may be owing to less equal treatment of part-time workers in the labour market.

Across four of the six estimators, there are also signs of a decrease in unequal treatment for full-time employees in the 1946 cohort between 1972 and 1977, over which period the Equal Pay Act came into force. Joshi and Newell (1987) found a decrease in

[7]This is likely ot be owing to some measurement error in the 1946 cohort experience variables (see chapter 4) as well as to smaller sample sizes.

Table 7.13: Estimates of unexplained log difference in pay for women and men full-time employees using different estimators, by survey

	1946 cohort			*1958 cohort*			*1970 cohort*		
Age at survey	*26*	*31*	*43*	*23*	*33*	*42*	*26*	*30*	*34*
(1)	0.30	0.23	0.28	0.17	0.13	0.19	0.09	0.10	0.09
	(0.02)	(0.03)	(0.04)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
(2)	0.28	0.24	0.32	0.15	0.16	0.20	0.08	0.10	0.09
	(0.02)	(0.03)	(0.08)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)
(3)	0.29	0.24	0.27	0.16	0.14	0.20	0.08	0.10	0.10
	(0.02)	(0.03)	(0.04)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
(4)	0.12	0.28	0.41	0.14	0.20	0.22	0.09	0.08	0.06
	(0.06)	(0.06)	(0.13)	(0.01)	(0.02)	(0.04)	(0.01)	(0.01)	(0.02)
(5)	0.26	0.18	0.29	0.17	0.14	0.19	0.08	0.10	0.12
	(0.03)	(0.05)	(0.07)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
(6)	0.24	0.30	0.19	0.16	0.17	0.19	0.07	0.09	0.12
	(0.02)	(0.04)	(0.13)	(0.01)	(0.02)	(0.05)	(0.02)	(0.02)	(0.03)
raw log gap	0.29	0.25	0.39	0.16	0.17	0.30	0.08	0.09	0.10
N (women)	526	243	450	2,813	1,673	2,277	2,419	2,736	1,773
N (men)	1,383	974	890	3,651	3,584	3,879	2,334	4,120	3,223

Notes: see table 7.11 for description of each of the 6 estimators. Standard errors are reported in parentheses.

unequal treatment for full-time employees over this period. The two estimators which do not show this pattern are estimator (4), which weights the parameter difference by the mean characteristics of the female population, and estimator (6), which compares women's mean full-time wage to the mean full-time wage for a weighted sample of men (with a similar distribution of propensity scores to women in full-time work). Arguably, less emphasis should be placed on the results from estimator (4), as discussed in the previous section, since the estimates are affected by the the arbitrary underweighting of the female advantage in returns to job tenure at age thirty-one, biasing the estimate upward. The result using estimator (4) though weakens this evidence of a decrease in unequal treatment within this cohort.

There is no clear pattern of change in unequal treatment with age from the trends for full-time employees. However, the interpretation of within-cohort trends is complicated by the fact that different groups of women are in full-time work at different ages. For the 1958 cohort, most of the estimates suggest either a decrease or no change in unequal treatment for full-time employees between the ages of twenty-three and thirty-three

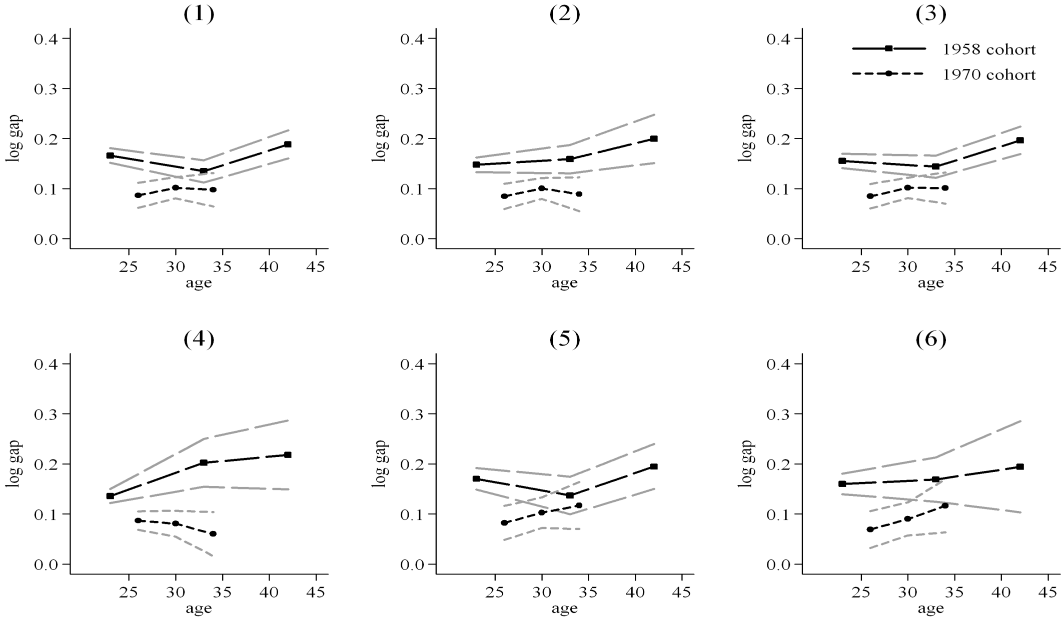

Figure 7.5: Estimates of unexplained log pay gap for full-time employees in the 1958 and 1970 cohort, by method.
The lighter, dashed lines show the 95% confidence intervals.

and an increase in unequal treatment between the ages of thirty-three and forty-two. However, the estimates weighted by the sample of all women's characteristics (not just those of full-time employees) show an increase in unequal treatment between the ages of twenty-three and thirty-three, but not between the ages of 33 and 42. It may be that women who have characteristics that attract unequal treatment are more likely to be out of work or in part-time work at age 33 and to have returned to full-time work by the age of 42. This interpretation is supported running models for a restricted sample of women and men who were in full-time work at both ages 33 and 42. For this restricted sample, there is no evidence of an increase in unequal treatment between the ages of thirty-three and forty-two (result not shown). Joshi et al. (2007) found the opposite result: they found an increase in estimated unequal treatment for the sample of 989 women who were in full-time work at both ages. One difference between the method they used and the method used here is that they estimated the OLS model using the whole cross-section samples of full-time employees at each age and used the parameters from these models (based on complete samples) to calculate indices of unequal treatment for the sub-sample of women in full-time work at both points, changing only the weighting of the parameters. In contrast, I restricted the sample to those working full-time at both ages thirty-three and forty-two for the purposes of estimating the OLS model (which has a considerably higher constant term when estimated for the restricted sample) and for the purpose of weighting the predicted wages.

Conclusions

There are several conclusions that are robust to different methods of measuring unequal treatment for the cross-section samples:

1. There has been an decrease in unequal treatment across the three cohorts in their twenties and thirties.
2. Unequal treatment persists for the 1970 cohort.
3. Unequal treatment is higher for all employees (including part-time employees) than for full-time employees in their thirties and forties.

The last of these results suggests that it is part-time working at older ages, rather than age per se, that attracts greater unequal treatment. Consistent with this, previous spells in part-time work have a negligible or negative impact on full-time pay, in contrast to consistent positive returns to full-time experience (table 7.5). An alternative interpretation is that women who choose to work part-time are less productive and motivated than other employees, in ways not captured by the characteristics included in the model. As Gregory and Connolly (2008) have pointed out, this second account is weakened by the fact that such a large fraction of women work part-time, including younger cohorts with high levels of education.

A limitation of the results based on cross-sections of employees at each survey is that within-cohort trends may arise from compositional effects (changes in the sample), induced by selection into and out of work, and full-time work, rather than changes in the relative pay of the same individuals over time. The majority of female employees were working full-time in their twenties, but only between 40 and 60 per cent by the their thirties. The next section of this chapter looks at estimates from longitudinally matched samples. This makes it possible to look directly at wage trajectories and to avoid some of the bias related to compositional effects. However, the samples are much smaller and women who are strongly attached to the labour market are over-represented in these samples. So, although the estimates are likely to be more internally valid, they may be less precise and less generalisable.

7.3 Gender differentials in pay for longitudinal matched samples

A second exercise involved comparing life-cycle trends in employment and pay for longitudinal samples of women and men who started their working lives with similar characteristics and levels of education. The focus of this exercise is on life-cycle trends, rather than cross-cohort trends. The reason for restricting the sample is to look at differences in wage growth of women and men, rather than the combined effects of changes in relative wages and changes in workforce composition. The estimates of unequal pay are simple differences in mean log wages for the same group of women and men at different ages. The longitudinal samples were out of necessity restricted to women and men who had been in work at each survey. As a consequence, the samples represent a select group of women with higher qualifications and stronger labour market attachment than average.

Trends in pay gaps were estimated for three longitudinal samples:

1. women and men who were in work at each survey;
2. women and men who were in full-time work at each survey; and
3. women who did not have children by the most recent survey and men with similar educational characteristics (but who may or may not have had children).

Table 7.14 shows the sample sizes for the three longitudinal samples. The samples include all women who meet the specified inclusion criteria and a sub-sample of men with similar qualifications and childhood backgrounds, matched one-to-one, without replacement.

Individuals were matched on the propensity score estimated from a probit model including maths and reading ability scores, childhood characteristics and highest qualfication attained by age 23 or 26. Table 7.15 summarises the probit models used to estimate propensity scores. It is qualifications achieved by the early twenties that does the work in these models. The substantial minority of women who are in work at each survey age represent a relatively highly qualified group and are more likely than men to have diploma or degree level qualifications. Using the estimated propensity score, a subsample of men who met the inclusion criteria (i.e. in work at each survey/in full-time work at each survey) were selected. The selection was on the basis of nearest-neighbour matching on the propensity score, without replacement. The resulting samples contain equal numbers of men and women. The results from matching with replacement, using a full weighted sample of male employees as the comparison group, gave the same results (with slightly smaller standard errors).

Table 7.14: Sample sizes for matched longitudinal samples, by cohort

	1958 cohort		1970 cohort	
	Men (N)	Women (N)	Men (N)	Women (N)
Sample 1				
Women: Employed at each survey	1,392	1,392	1,451	1,451
Men: Employed at each survey *and* have a similar distribution of qualifications to female sample				
Sample 2				
Women: Employed full-time at each survey	596	596	853	853
Men: Employed full-time at each survey *and* have a similar distribution of qualifications to female full-time sample				
Sample 3				
Women: Employed at each survey and not had children by most recent survey	422	422	669	669
Men: Employed at each survey *and* have a similar distribution of qualifications to female childless sample				

Table 7.15: Estimated probit parameters from models used in propensity-score matching for longitudinal samples

	Sample 1: Employees		Sample 2: Full-time employees		Sample 3: Women without children	
	1958 cohort	*1970 cohort*	*1958 cohort*	*1970 cohort*	*1958 cohort*	*1970 cohort*
Mother's age at birth in quartiles (youngest = ref)						
Second quartile	-0.02 (0.06)	-0.05 (0.07)	-0.05 (0.09)	+0.05 (0.09)	+0.03 (0.09)	+0.18 (0.09)
Third quartile	-0.04 (0.07)	-0.02 (0.08)	0.00 (0.09)	+0.11 (0.09)	+0.04 (0.10)	+0.18 (0.10)
Oldest quartile	+0.06 (0.07)	-0.04 (0.08)	+0.11 (0.09)	+0.11 (0.10)	+0.28 (0.10)	+0.29 (0.10)
(missing)	+0.01 (0.12)	+1.11 (0.56)	0.00 (0.14)	+0.81 (0.56)	-0.03 (0.16)	+6.05 (0.19)
Father's social class (V & VI = ref)						
I	-0.01 (0.12)	-0.29 (0.13)	-0.03 (0.16)	-0.23 (0.15)	+0.01 (0.17)	-0.28 (0.15)
II	+0.10 (0.08)	-0.11 (0.09)	+0.12 (0.11)	-0.13 (0.11)	+0.09 (0.12)	-0.21 (0.12)
III	+0.16 (0.09)	-0.23 (0.11)	+0.19 (0.11)	-0.19 (0.13)	+0.21 (0.12)	-0.21 (0.13)
IV	+0.12 (0.06)	-0.09 (0.08)	+0.18 (0.08)	-0.03 (0.10)	+0.16 (0.09)	-0.14 (0.10)
(missing)	+0.08 (0.08)	-0.47 (0.20)	+0.25 (0.10)	-0.44 (0.23)	+0.26 (0.11)	-0.68 (0.26)
Mother's schooling (minimum = ref)						
Left at 17	+0.08 (0.12)	+0.04 (0.10)	+0.02 (0.16)	0.00 (0.11)	+0.05 (0.17)	-0.12 (0.12)
Left at 18 or older	+0.18 (0.12)	-0.01 (0.10)	+0.22 (0.15)	-0.02 (0.12)	+0.03 (0.17)	-0.17 (0.13)
(missing)	-0.03 (0.18)	-1.31 (0.56)	-0.09 (0.22)	-0.97 (0.55)	-0.17 (0.25)	d
Father's schooling (minimum = ref)						
Left at 17	0.00 (0.13)	+0.05 (0.10)	+0.02 (0.16)	-0.01 (0.12)	+0.09 (0.17)	+0.09 (0.13)
Left at 18 or older	-0.01 (0.11)	+0.15 (0.10)	-0.03 (0.14)	+0.11 (0.12)	-0.05 (0.15)	+0.21 (0.12)
(missing)	+0.21 (0.14)	+0.47 (0.20)	+0.17 (0.18)	+0.57 (0.23)	+0.07 (0.20)	+0.72 (0.25)

Table 7.15: Estimated probit parameters from models used in propensity-score matching for longitudinal samples - continued

	Sample 1: Employees		Sample 2: Full-time employees		Sample 3: Women without children	
	1958 cohort	*1970 cohort*	*1958 cohort*	*1970 cohort*	*1958 cohort*	*1970 cohort*
Siblings (4+ = ref)						
Only child	+0.11 (0.13)	+0.16 (0.19)	+0.37 (0.15)	+0.24 (0.22)	+0.36 (0.17)	+0.25 (0.23)
One sibling	0.11 (0.09)	+0.03 (0.17)	+0.20 (0.12)	0.12 (0.20)	+0.27 (0.14)	+0.27 (0.25)
Two or three siblings	+0.01 (0.08)	-0.04 (0.16)	+0.03 (0.10)	0.00 (0.19)	+0.19 (0.11)	+0.30 (0.22)
(missing)	-0.10 (0.16)	+0.03 (0.17)	0.00 (0.21)	+0.07 (0.20)	+0.35 (0.23)	+0.25 (0.23)
Older siblings (ref = 2+)						
No older sibling	0.00 (0.09)	-0.05 (0.10)	-0.11 (0.11)	+0.01 (0.11)	+0.02 (0.12)	+0.01 (0.12)
One older sibling	-0.04 (0.08)	0.00 (0.09)	-0.11 (0.10)	+0.02 (0.11)	-0.04 (0.11)	-0.03 (0.11)
Maths score at age 11	+0.05 (0.04)	-0.32 (0.04)	+0.07 (0.05)	-0.34 (0.05)	+0.04 (0.05)	-0.33 (0.05)
Reading score at age 11	-0.03 (0.04)	+0.28 (0.04)	-0.01 (0.05)	+0.30 (0.05)	0.00 (0.05)	+0.28 (0.05)
Missing maths score	+0.03 (0.07)	-0.06 (0.06)	+0.07 (0.09)	-0.03 (0.07)	+0.05 (0.09)	-0.05 (0.08)
Highest qualification at age 26 or 23 (no quals = ref)						
O-level or equivalent	+0.15 (0.07)	+0.33 (0.07)	+0.16 (0.09)	+0.41 (0.09)	+0.07 (0.09)	+0.39 (0.10)
A-level or equivalent	-0.36 (0.08)	+0.03 (0.09)	-0.12 (0.10)	+0.24 (0.11)	-0.26 (0.11)	+0.16 (0.12)
Diploma	+0.29 (0.08)	+0.33 (0.10)	+0.38 (0.11)	+0.51 (0.11)	+0.19 (0.12)	+0.39 (0.12)
Degree or higher	+0.08 (0.10)	+0.21 (0.09)	+0.26 (0.12)	+0.50 (0.10)	+0.15 (0.14)	+0.44 (0.11)
Constant	-0.41 (0.10)	-0.06 (0.18)	-1.07 (0.13)	-0.77 (0.21)	-1.39 (0.15)	-1.02 (0.23)
Pseudo R^2	0.02	0.03	0.03	0.04	0.03	0.04
Sample size	3,357	2,956	2,507	2,329	2,386	2,172
Women (N)	1,392	1,451	596	853	422	699
Men (N)	1,965	1,505	1,911	1,476	1,964	1,503

The treatment group in each model is the female sample that meets the inclusion criteria. Positive signs on coefficients show that a characteristic is more common in the female compared to the male sample.

All employees

Figures 7.6 shows an increase in the gender log pay gap with age for both the cohorts over their twenties and thirties. The increase in the gap is statistically significant. However, there is quite a lot of variation in wage trajectories across different women and men. Figure 7.7 shows the distribution of gender differences in wage growth across individual matched pairs (of women and men). Although the median male advantage in wage growth is positive (and statistically significant), there a substantial fraction (up to 40%) of cases in which women's wages increase more than those of similarly qualified (matched) men. However, for this group, women started out with considerably lower wages in their twenties. The analysis here misses out early-career inequalities in wage growth, which Manning and Petrongolo (2008) found to be important.

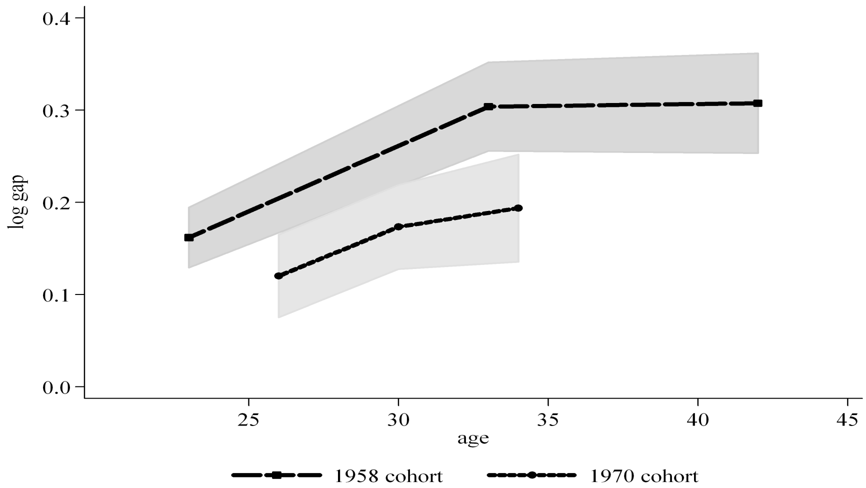

Figure 7.6: Log pay gap for longitudinal matched samples of employees in the 1958 and 1970 cohort
The shaded areas show the 95% confidence intervals.

Women with lower wage growth than their matched, male counterparts are more likely to have switched to part-time hours than those with similar or higher wage growth. In the 1958 cohort, more than two fifths of women who saw a lower increase in their log wages between the ages of 23 and 33 switched from full into part-time work and 7 per cent were in part-time work at both ages. In contrast, more than 70 per cent of women who saw similar or higher wage growth were in full-time work at both ages. In the 1970 cohort, around 60 per cent of women with lower wage growth remained in full-time work at both points, compared to 66 per cent of women with similar or higher

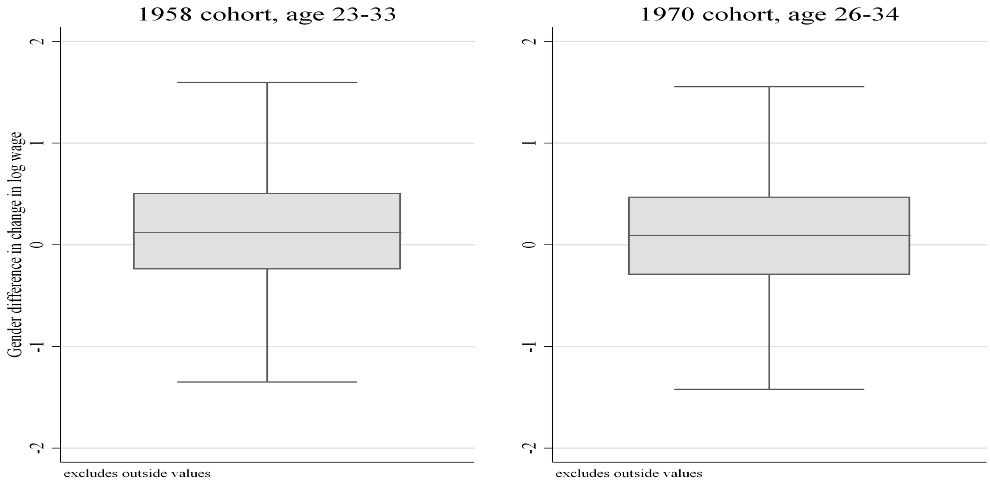

Figure 7.7: Distribution of gender differences in wage growth (1)
(1) Wage growth is defined as the change in log wages with age. The gender difference = (male change) - (female change). The central horizontal line shows the median difference. The box shows the 25th and 75th percentiles.

wage growth.

Full-time employees

For the samples who were in full-time work at all three survey ages, there is no increase in the average gender gap with age for either the 1958 or the 1970 cohort. The unexplained gap in log pay is positive and statistically significant at each age, but there is no widening of the gap at all for the 1958 cohort between the ages of 23 and 42. For the 1970 cohort, the slight male advantage in wage growth between the ages of 26 and 34 for the 1970 cohort is not statistically significant at the 5% level. The same pattern is observed for the sample restricted to women without children (which is largely overlapping with the longitudinal sample of full-time employees).

Again, it should be emphasised that the ages at which wages are shown for the cohorts do not cover the period straight after labour market entry and Manning and Petrongolo (2008) show evidence that the gender gap in pay, including for full-time employees and childless women, emerges over the first ten years in the labour market. The evidence presented here shows significant unexplained pay gaps by the ages of 23 and 26 and is consistent with their finding.

7.4 Conclusions

There are several important findings that emerge from the analysis of pay gaps within and across the cohorts. First, the estimates suggest that unequal treatment persisted for the 1970 birth cohort by the age of 34 (in 2004). Women in this cohort who had worked full-time and not had children by the age of 34 were paid systematically less than similarly qualified men. Although some of this unexplained gap may be owing to differences in women's and men's job and occupation choices, this in itself raises a question about why women should systematically choose jobs that pay less or, on the flip side, why the jobs that many women do, are paid less than those done by similarly-qualified men. Given that nearly a third of women in the cohort had not had children by this age and many are likely to remain childless (Kneale and Joshi, 2008), it is not plausible to attribute all of this unexplained gap to the fertility intentions and associated employment choices.

Second, the estimates presented in this chapter provide evidence of a substantial decrease in unequal treatment across the three birth cohorts. The unexplained gap in pay has reduced across the cohorts when in their twenties and thirties. This finding is robust to a range of different ways of measuring unequal treatment, for the samples including all employees and the restricted sample of full-time employees. The cross-cohort change in the wage effects of part-time working is much harder to assess. The majority of women in each cohort (just over half in the 1970 cohort) have worked part-time at some point in their life. However, the composition of the part-time workforce changes with age and the extent and patterns of part-time working have changed considerably across the three cohorts. The increase in mother's employment rates have mostly been through an increase in part-time working. To quantify the changes in the impact of part-time working for each of the cohorts, it would be important to adjust for compositional effects. This would be a separate and substantial piece of work.

Third, the analysis of wage patterns for longitudinal samples of the 1958 and 1970 cohorts show that the gender pay gap widened over their twenties and thirties. The decrease in women's relative pay was linked to having children and switching to part-time work. Women who worked full-time for most of their careers were paid systematically less than similarly-qualified men, but did not face a decrease in their relative pay over their twenties and thirties. In contrast, women who switched to part-time hours faced significantly lower growth in their hourly earnings. The evidence from wage models for the cross-sections of women working full-time in their forties also showed that part-time working has long-term negative effects on pay.

Chapter 8

Summary and conclusions

This thesis opened with the questions of how unequal labour market opportunities affect women's and men's relative pay over their working lives and with how inequalities have changed across three British generations since the 1970s. It has presented new evidence on changes in women's and men's relative pay opportunities across three British birth cohorts. It has also contributed new estimates of unequal treatment to the existing body of evidence for these cohorts. Finally, it has used the longitudinal aspect of the cohort earnings data for the first time to look at how gender inequalities cumulate over the life-cycle. This concluding chapter draws together the research findings from chapters 5 and 7 and reflects upon what is added to the existing quantitative literature on gender inequalities. The final parts of the chapter consider prospects for the future and the role for policy in shaping future trends.

8.1 Trends in women's and men's pay opportunities

The first analysis focused on a broad measure of women's and men's unequal pay opportunities. The question addressed was how women's relative pay opportunities, including those of non-workers, had changed across the three cohorts. The motivation for this investigation was the simultaneous change in women's rates of pay and rates of employment, and the concern that, by looking only at wages of employees, we may get a partial and distorted picture of pay opportunities.

Blundell et al. (2007) addressed this question using cross-section data from the Family Expenditure Survey (FES) for 1978 and 1998. They investigated the effects of changes in workforce composition, induced by changes in patterns of employment, on gender differentials and also on the picture of wage inequality overall and on educational differentials. They used a variety of different assumptions about the relative potential wages of non-workers and workers to estimate upper and lower bounds on differentials in median potential pay. Two assumptions were critical to their findings on gender differentials: first, that the median wages of workers were likely to be higher than those of non-workers for a fixed age and level of education (a weak form of positive selection); and second, that changes in out-of-work benefits entitlements affected employment participation but not pay (an exclusion restriction). They concluded that changes in workforce composition masked some of the improvement in women's labour market position between 1978 and 1998 for younger, less educated workers. This question has not been examined before using the British birth cohort data. This provided the opportunity to look at trends over a longer period, from before the Equal Pay Act came into force up to 2004. The detailed information on childbearing and job histories also made it possible to avoid the assumption of positive selection into work.

Across the 1946 and 1970 cohorts, women's median hourly pay rose from around 60% of men's to around 80% of men's, when in their early thirties. Simultaneously, the proportion of women in paid employment rose from around a half to three-quarters. The working hypothesis was that low pay offers were more likely to be rejected and that the extent of unequal pay opportunities for women in the two earlier cohorts could be partly masked by lower rates of employment. This was the essence of the result in Blundell et al. (2007) and from work by Blau and Kahn (2006a) for the US. However, another hypothesis suggested by the literature for the US was that changes in women's patterns of employment participation meant that improvements in women's labour market opportunities were overstated, not understated, in wage trends (Mulligan and Rubinstein, 2008).

The cohort datasets contain detailed information on employment and childbearing histories, but little information that would lend itself to suitable exclusion restrictions. On this basis, I took the approach of imputing potential wages for non-employed women and men based on the actual wages of similar, but employed, women and men of the same cohort and age and with similar employment and family histories (up to that point). It was not necessary to make assumptions of positive selection into work. It was necessary, though, to make the assumption of 'selection on observables'. In other words, it was assumed that individuals with the same characteristics shared the same potential wage, whether or not they were actually in work. This assumption was considered to be reasonable in light of the detailed information on qualifications, employment and childbearing histories and childhood maths and reading ability. Possible bias arising from unobserved selectivity was also investigated using the data on maths and reading ability and, for the 1958 cohort, on wages in a first job. On balance, the evidence supported the assumption, but with some evidence that the estimated composition effects could be overstated owing to some negative unobserved selectivity bias in the estimates for the 1946 and 1958 cohorts at younger ages, and the reverse (positive selection) for the 1958 and 1970 cohorts at older ages.

It also became clear from the analysis of the cohort data, that composition effects are linked to variation in the timing of childbirth and patterns of return to work amongst mothers. For women in the 1946 cohort, around 80% had become mothers by their late twenties and nearly all mothers spent several years out of the workforce. At the ages of 26 and 31, around half of women were in work, but these were different groups of women. Changes in composition came mainly from social and educational differences in the timing of childbirth over a relatively short number of years, and less from variation amongst mothers in patterns of return to work. For women in the 1958 and 1970 cohorts, women tended to have children later and more mothers returned to work more quickly after having children. Childbearing years were more spread out and variation in both the timing of childbirth and patterns of return to work increased. However, composition effects on wages were not so important in the later cohorts simply because the majority of women were in work at each age.

The broad picture was of an improvement in women's position in the labour market in Britain since the 1970s, including an improvement in their underlying pay opportunities relative to men's. The main finding was that the cross-cohort improvement in women's labour market opportunities was, if anything, understated in the increase in average pay for employed cohort members at younger ages. This result is consistent with Blundell et al. (2007).

8.2 Trends in unequal pay for equally qualified women and men

Turning to the question of unequal remuneration of women's and men's skills in the labour market, the second piece of work was based directly on previous work on the birth cohorts by Joshi and Newell (1989), Joshi and Paci (1998), Makepeace et al. (1999) and Joshi et al. (2007). Previously, evidence had been found of systematic unexplained gender differences in pay in each of the three cohorts, not accounted for by gender differences in education or labour market experience. Further, previous results suggested a marked decrease in unequal treatment across the three cohorts.

Following Joshi and Paci (1998), I used standard decomposition methods to estimate unexplained gender differentials in log pay. I used as yet unused surveys of the 1946 and 1970 cohorts, at ages 43 (in 1989) and 34 (in 2004) respectively to make new estimates of unequal treatment for the cohorts at these ages. I also used propensity-score matching models to test the sensitivity of the results to different ways of estimating counterfactual 'non-discriminatory' wages for individuals. I analysed gender differentials separately for the whole samples of employees (including part-time employees) and for restricted samples of full-time employees.

For each of the cohorts at every age, I found a statistically significant difference in the average (log) pay of women and men with similar levels of education and employment experience. Even for the 1970 birth cohort, in which women and men have similar levels of education, there was a 10 per cent unexplained difference in average pay by the age of 26. In this cohort, women who were working full-time at the age of thirty-four (in 2004) were also paid 10 to 12 per cent less than men with similar levels of education and experience. This is in line with previous estimates for this cohort at age 30 (Joshi et al., 2007).

I also found strong evidence of a cross-cohort decrease in the unexplained gender pay gap. The log differential in pay between women and men for similar levels of education and experience fell from around 0.3 for the 1946 birth cohort at age 31 to around 0.2 for the 1958 birth cohort at age 33 to around 0.1 for the 1970 birth cohort at age 30. Comparing the relative pay just for full-time employees also shows a cross-generational decrease in unequal treatment. Again, this finding is in line with previous results for the cohorts (Joshi and Paci, 1998; Makepeace et al., 1999; Joshi et al., 2007).

Comparing estimates for all employees to estimates for full-time employees revealed that lower pay associated with part-time working. However, the cross-cohort change in the wage effects of part-time working are not clear from the present analysis. The majority of women in each cohort (just over half in the 1970 cohort) worked part-time at some point over the course of their working lives. However, patterns of part-time working have changed considerably across the three cohorts. Many women in part-time work around childbearing ages in the 1970 cohort simply would not have been in work in the 1946 cohort. To quantify the changes in the impact of part-time working for each of the cohorts, adjusting for compositional effects, would be a separate and substantial piece of work.

The snapshots based on wages of cross-sections of employed cohort members suggested a widening of the unexplained pay gap with age for the 1958 and 1970 cohorts. A limitation of the results based on cross-sections of employees at each survey is that within-cohort trends may arise from compositional effects (changes in the sample), induced by selection into and out of full-time work, rather than changes in the relative pay of the same individuals over time. The way in which these compositional issues are handled affects the estimate of unequal treatment and I obtained different results from Joshi et al. (2007) as a consequence. They estimated an increase in unequal treatment for full-time workers in the 1958 cohort between the ages of 33 and 42, whereas I did not. Although they restricted their sample to the 989 women in full-time work at both ages, the wage model parameters were estimated on the complete cross-sections in full-time work. In contrast, my results were based on the restricted sample for both purposes. The important point is that there are lower paid workers who return to full-time work in their forties from either part-time work or non-employment. The way that these workers feature in estimates makes a difference to the assessment of unequal treatment.

8.3 Life-cycle trends in unequal pay

The third area of investigation was life-cycle trends in gender pay differentials. For this, I started from work by Manning and Petrongolo (2008) on gender differences in wage growth early on in the career and by Brewer and Paull (2006) and Connolly and Gregory (2009) on the longitudinal wage effects of motherhood and part-time working. I used hourly wages collected from the 1958 and 1970 cohorts in their twenties and thirties and, for the 1958 cohort, in their early forties. Most cohort members had been in work for several years by their early to mid twenties (up to a decade for the 1970 cohort by age 26) so my analysis covers a later period of the life-cycle to Manning and Petrongolo (2008) and also goes back a decade earlier (to 1981). The focus on longitudinal trends increases the internal validity of the results, but at the cost of generalisability, since women who are strongly attached to the labour market, who are over-represented in the sample, are a select and more highly-qualfied group.

I used the longitudinal aspect of the cohort datasets to investigate changes in the gender diferential for the same group over time. The cohort studies allow the possibility of selecting large samples of the same age and tracking their wages at several points in time. However, such a small minority of women in the 1946 cohort were employed at each age that no informative changes could be estimated for this cohort. For the

1958 and 1970 cohorts, around a third of women were in paid work at all three survey ages. Within both cohorts, there was a substantial and significant widening of the gender gap in pay over the life-cycle for women and men who started with similar qualifications and family backgrounds. However, there was variation in wage growth and women experienced lower or negative rates of wage growth were much more likely to have switched to part-time work between their twenties and thirties. The effects of this switch persisted into their forties for the 1958 cohort, at which age a substantial number had returned to full-time work. This finding is consistent with other evidence of lower pay in part-time jobs (Manning and Swaffield, 2008) and of long-term negative effects of part-time working on occupational mobility and wages (Connolly and Gregory, 2009).

Women who were in full-time employment at all three survey ages, and women without children by the most recent survey, were paid systematically less than similarly qualified men. However, they did not experience the same decrease in relative pay with age. Given that these are small and highly-qualified samples, it is harder to generalise about the protective benefits of remaining in full-time work on women's relative pay.

One interpretation of this difference in results is it is part-time working at older ages, rather than age per se, that attracts greater unequal treatment. Consistent with this, previous spells in part-time work have a negligible or negative impact on full-time pay, in contrast to consistent positive returns to full-time experience. An alternative interpretation is that women who work part-time reap benefits other than pay from their jobs and/or that they do less demanding work.

An interesting future piece of work could be done modelling wage patterns for larger samples, including individuals with some non-employment and missing wage data. This would require some fairly complex models of missingness and some strong assumptions about selection into and out of work.

8.4 Prospects for a more equal society

The persistence of systematic differences in the pay of equally qualified and experienced women and men is strong evidence of unequal treatment in the British labour market. Particularly powerful is the evidence from the 1970 birth cohort, in which women and men with equal levels of education and similar employment histories are still paid differently. Further, the increase in the gender pay gap between the ages of 26 and 34 suggests that unequal pay will not disappear as a matter of course, but rather that current generations of young women and men are likely to face more, not less, inequality as they grow older.

The interpretation offered here of what are historically small gender differences in pay remains disputed though. Brewer and Paull (2006) find a differential of 11 per cent between men's and women's hourly wages prior to having children and controlling for family background, work characteristics, employment experience and occupation, similar to the estimate for the 1970 cohort presented in this study. However, rather than viewing this as important evidence of gender discrimination, the authors suggest that small, pre-children gender pay differences represent 'anticipatory effects of the impact of children' or that are driven by factors of less relevance than the much larger effects of raising children on pay (Brewer and Paull, 2006, p.3). Joshi and Paci (1998) conclude rather differently: 'Neither taking maternity leave nor remaining childless permit women to escape the financial penalty of being female' (p.137).

Another way to think about small gender differences is in terms of their cumulative impacts. Becker (1991) has argued that small initial differences in women's and men's capacity to care for children contributes to their specialisation in 'home' or 'market' work. The same logic could be applied to arguments about discrimination. Under this view, small discriminatory differences in women's and men's pay and treatment in the labour market provide financial incentives and influence cultural norms about who cares for children, also with cumulative life-cycle impacts.

On the positive side, the cross-generational increase in women's pay opportunities and the decrease in unequal pay for women and men with similar levels of skill shows that unequal pay is not immutable. Arguably, the reduction in gender discrimination in the British labour market following the implementation of the Equal Pay Act had cumulative positive effects both on women's pay and employment and also on the investment behaviour of subsequent generations of women. Joshi et al. (1985), Neuburger (1984), Joshi and Newell (1987) and Manning (1996) have also drawn attention to the increase in women's pay and employment, following the introduction of the Equal Pay Act. The fact that women's pay, their rates of employment, education and relative pay for the same levels of education have increased together is more supportive of a view that institutional and market reductions in inequality are mutually reinforcing than of a view that markets adjust in an automatic or negative way to policy interventions.

The change in attitudes to women's employment since 1946, when the first cohort was born, is striking. The long term effects of equal pay legislation on social norms and attitudes about women's work may also be important. In her submission to the

Royal Commission on Equal Pay in 1946, three decades before the implementation of any equal pay legislation, Barbara Wootton wrote,

> Absence of discrimination between male and female wages might be an important secondary influence in undermining the convention of female inferiority. Lower pay is an obvious badge of inferiority and without this label candidates for jobs might be more likely to be considered on personal merits. The word 'secondary' in this context should, however, be stressed. Equalisation of pay will not itself induce a different attitude towards the place of women in society: its significance would be that it might reinforce a half-formed belief that the present method of discrimination is unscientific, and out of keeping with modern standards.

Royal Commission on Equal Pay, 1946, p.194

There is the real prospect that policy can affect not just behaviour in the short-term but can shape attitudes in the long term, such that job-sharing, flexible working and reasonable minimum rates of pay could become norms in the workplace rather than being perceived as atypical, costly or inconvenient.

8.5 Policy for a more equal society

Three areas of policy are important for the position of women and men in work. The first area is direct anti-discrimination legislation. The evidence from the present study is supportive of the view that the anti-discrimination laws introduced in 1975 have had significant and lasting impacts on women's pay opportunities and treatment in the workplace. This legislation was bold, compulsory for employers and implemented largely via collective bargaining structures. The Equality Bill was published on 27th April 2009 and includes measures aimed at promoting transparency in relation to pay, including requiring large employers (with more than 250 employees) to publish information about the gender pay gap. There is continued debate about the likely practical impact of these measures.

A second important area is that of policy on parental leave and policies to support flexible working. The evidence from the cohorts is that periods out of work and in part-time work damage women's long-term pay prospects. What remains unclear though from the evidence presented here is how far the decline in relative pay associated with breaks in work and part-time working is owing to loss of skills and decisions to seek less demanding and responsible positions on a return to work and how far it is owing to less favourable treatment and opportunities. Connolly and Gregory (2008) have drawn attention to the importance of job changes and occupational downgrading as routes through which women's pay is damaged upon a move from full-time to part-time hours. The implication of this is that strengthened rights to parental leave, strengthened rights to reinstatement and to return to work part-time with the same employer would help to sustain women's levels of pay. On the other hand, strengthening rights for mothers and not fathers may lead to more employer discrimination against women of childbearing ages (Ruhm, 1998), as well as preventing fathers from taking a fuller role in raising children. In a report for the Department of Work and Pensions, Brewer and Paull (2006) concluded that encouraging mothers to return to paid work between births, not raising maternity pay and supporting the provision of in-work childcare are the right ways to tackle the gender pay gap. Joshi (2002) has pointed out that parental work also requires public support and that a rebalancing of parental responsibilities amongst fathers and mothers is possible in other ways than through the shift of childcare into the sphere of paid work.

The final important area of policy is that of broader interventions affecting labour market institutions, public sector pay and wage structures. With a recession underway, employers are likely to fall back on short-termist and traditional working practices and employees are less likely to make pay or other demands for fear of losing their jobs. Action in three policy areas could make a real difference. The rate at which the National Minimum Wage is set disproportionately affects women's pay. If low pay is viewed partly as the result of a discriminatory or monopsonistic labour market, a view supported by the evidence in this thesis, the implication is that an increase in the minimum wage does not necessarily reduce employment. Further, better pay in sectors such as care work could contribute to their professionalization. Policy on public sector employment and pay matters, with a third of women employees working in the public sector, and nearly 17 per cent of the male workforce (Labour Force Survey Historical Quarterly Supplement, Q2 2009). Policies that affect bonuses and high pay matter, with only a tiny minority of women working in the most highly-paid jobs. Policies that tend to make the workplace more equal overall tend to make it more equal for women and men.

As argued at the start of this thesis, pay is a marker of status, skill, responsibility and power in the workplace. As women's pay has increased across three British generations, their attachment to the labour market has increased and so have the educational aspirations and attainments of girls in successive generations. Taken together, the evidence from the birth cohorts suggests that the effects of decreases in labour market inequality are cumulative and self-reinforcing. The significant and lasting shift toward more equal pay following the introduction of the 1970 Equal Pay Act is compelling evidence of the effectiveness and importance of legislation in reducing inequality.

Bibliography

Daron Acemoglu and Jörn-Steffen Pischke. Beyond Becker: Training in imperfect labour markets. *The Economic Journal*, 109:F112–F142, February 1999.

George A. Akerlof. A theory of social custom, of which unemployment may be one consequence. *Quarterly Journal of Economics*, 94:749–745, 1980.

George A. Akerlof and J. L. Yellen. *Efficiency wage models of the labour market*. Cambridge University Press, Cambridge, 1986.

James Albrecht, Aico van Vuuren, and Susan Vroman. Decomposing the gender wage gap in the Netherlands with sample selection adjustments. IZA DP No.1400, Institute for the Study of Labor, November 2004.

James W. Albrecht, Per-Anders Edin, Marianne Sundström, and Susan B. Vroman. Career interruptions and subsequent earnings: A reexamination using Swedish data. *The Journal of Human Resources*, 34(2):294–311, Spring 1999.

T Anderson, J Forth, H Metcalf, and S Kirby. The Gender Pay Gap. Cabinet Office, Women and Equality Unit, London, 2001.

Joshua D. Angrist and Alan B. Krueger. Instrumental variables and the search for identification: from supply and demand to natural experiments. *Journal of Economic Perspectives*, 15(4):69–85, Fall 2001.

Joshua D. Angrist and Jörn-Steffen Pischke. *Mostly harmless econometrics: An empiricist's companion*. Princeton University Press, Princeton and Oxford, 2009.

Wiji Arulampalam. Is unemployment really scarring? Effects of unemployment experiences on wages. *The Economic Journal*, 111(475):F585–F606, November 2001. Features.

A. Atkinson, F. Bourguignon, and C. Morrison. *Empirical studies of earnings mobility.* Hanvood Academic Publishers, Reading, 1992.

David Autor and David Dorn. This job is 'getting old': measuring changes in job opportunities using occupational age structure. NBER Working Paper 14652, National Bureau of Economic Research, Cambridge, Massachusetts, January 2009.

Linda Babcock and Sara Laschever. *Women don't ask: negotiation and the gender divide.* Princeton University Press, Princeton, New Jersey, 2003.

Gary S. Becker. *The economics of discrimination*, volume 1st edition. University of Chicago Press, Chicago, 1957.

Gary S. Becker. *Human Capital: a theoretical and empirical analysis*, volume 1st edition. Columbia University Press, New York, 1964. For the National Bureau of Economic Research.

Gary S. Becker. Human capital, effort and the sexual division of labor. *Journal of Labor Economics*, 3(1):S33–S58, 1985. Part 2.

Gary S. Becker. *A treatise on the family.* Harvard University Press, Cambridge MA, 1991. expanded edition.

Yoram Ben-Porath. The production of human capital and the life-cycle of earnings. *The Journal of Political Economy*, 75(4):352–365, August 1967. Part 1.

D. Blackaby, A. L. Booth, and J. Frank. Outside offers and the gender pay gap: empirical evidence from the UK academic labour market. *The Economic Journal*, 115: 81–107, February 2005.

Jo Blanden, Paul Gregg, and Lindsey Macmillan. Accounting for intergenerational income persistence: noncognitive skills, ability and education. *The Economic Journal*, 117:C43–C60, March 2007.

Francine D. Blau and H. Beller, Andrea. Black-white earnings over the 1970s and 1980s: gender differences in trends. *The Review of Economics and Statistics*, 74(2):276–286, May 1992.

Francine D. Blau and Lawrence M. Kahn. Understanding international differences in the gender pay gap. NBER Working Paper 8200, National Bureau of Economic Research, April 2001.

Francine D. Blau and Lawrence M. Kahn. The U.S. Gender Pay Gap in the 1990s: slowing convergence. *Industrial and Labor Relations Review*, 60(1):45–66, October 2006a.

Francine D. Blau and Lawrence M. Kahn. The U.S. Gender Pay Gap in the 1990s: slowing convergence. NBER Working Paper 508, National Bureau of Economic Research, March 2006b.

Alan S. Blinder. Wage discrimination: reduced form and structural estimates. *The Journal of Human Resources*, 8(4):436–455, Autumn 1973.

David E. Bloom and Mark R. Killingsworth. Pay discrimination research and litigation: the use of regression. *Industrial Relations*, 21(3):318–339, Autumn 1982.

Richard Blundell, Lorraine Dearden, Alissa Goodman, and Howard Reed. The returns to higher education in Britain: evidence from a British cohort. *The Economic Journal*, 110:F82–F99, February 2000.

Richard Blundell, Howard Reed, and Thomas M. Stoker. Interpreting aggregate wage growth: the role of labour market participation. *American Economic Review*, 93(4): 1114–1131, September 2003.

Richard Blundell, Lorraine Dearden, and Barbara Sianesi. Evaluating the effect of earnings: models, methods and results from the National Child Development Study. *Journal of the Royal Statistical Society Series A*, 168(3):473–512, 2005. Statistics in Society.

Richard Blundell, Amanda Gosling, Hideko Ichimura, and Costas Meghir. Changes in the distribution of male and female wages accounting for employment composition using bounds. *Econometrica*, 75(2):323–363, March 2007.

Tito Boeri, Daniela Del Boca, and Christopher Pissarides, editors. *Women at work: An economic perspective*. Oxford University Press, Oxford and New York, 2005. A report for the Fondazione Rodolfo Debenedetti.

L. Booth, Alison, Marco Francesconi, and Jeff Frank. A sticky floors model of promotion, pay and gender. *European Economic Review*, 47:295–322, 2003.

Mike Brewer and Gillian Paull. Newborns and new schools: critical times in women's employment. Research Report No 308, Department for Work and Pensions, 2006. Research carried out by the Institute for Fiscal Studies.

Kenneth Burdett. A theory of employee job search and quit rates. *The American Economic Review*, 68(1):212–220, March 1978.

Kenneth Burdett and Dale T. Mortensen. Wage differentials, employer size and unemployment. *International Economic Review*, 39(2):257–273, May 1998.

Neville R. Butler and Dennis G. Bonham. *Perinatal mortality. The first report of the 1958 British Perinatal Mortality Study under the auspices of the National Birthday Trust Fund.* E. & S. Livingstone Ltd., Edinburgh and London, 1963.

Neville R. Butler, J. Golding, and B.C. Howlett. *From birth to five: A study of the health and behaviour of Britain's five-year-olds.* Pergamon Press, Oxford, 1986.

John Bynner and Harvey Goldstein. Three generations of children - an edited transcript of a video recording made in March 1982 of the then directors of the three major British cohort studies in conversation. *Paediatric and Perinatal Epidemiology*, 12 (Supplement 1):15–30, 1998.

John Bynner and Samantha Parsons. New Light on Literacy and Numeracy: Results of the Literacy and Numeracy Assessment in the Age 34 Follow-up of the 1970 Cohort Study (BCS70). Technical report, National Research and Development Centre for Adult Literacy and Numeracy (NRDC), 2005.

John Bynner, Elsa Ferri, and Peter Shepherd, editors. *Twenty something in the 1990's: Getting on, getting by, getting nowhere.* Ashgate Press, Aldershot, 1997.

John Bynner, Harvey Goldstein, and Eva Alberman. Neville Butler and the British Birth Cohort Studies. *Paediatric and Perinatal Epidemiology*, 12(Supplement 1): 1–14, 1998.

John Bynner, Neville Butler, Elsa Ferri, Peter Shepher, and Kate Smith. The design and conduct of the 1999-2000 surveys of the National Child Development Study and the 1970 British Cohort Study. CLS Cohort Studies Working Paper 1, Centre for Longitudinal Studies, Institute of Education, London, December 2001.

Centre for Longitudinal Studies. National Child Development Study 4: derived variables. Technical report, Centre for Longitudinal Studies, Institute of Education, London, UK, 1981.

Sara Connolly and Mary Gregory. The National Minimum Wage and hours of work for low paid women. *Oxford Bulletin of Economics and Statistics*, 64:607–631, December 2002.

Sara Connolly and Mary Gregory. Moving down: Women's part-time work and occupational change in Britain. *The Economic Journal*, 118(526):F52–F76, February 2008. Feature: women's part-time work.

Sara Connolly and Mary Gregory. The part-time pay penalty: earnings trajectories of British women. *Oxford Economic Papers*, 61(Supplement 1):i76–i97, April 2009. Special issue: women and wages.

Sara Connolly and Susan Long. Women in science - fulfilment or frustration? Evidence from the UK. Technical report, University of East Anglia, Norwich, April 2007.

Jeremiah Cotton. On the decomposition of wage differentials. *The Review of Economics and Statistics*, 70(2):236–243, May 1988.

Lorraine Dearden, Alissa Goodman, and Peter Saunders. Income and living standards. In Elsa Ferri, John Bynner, and Michael Wadsworth, editors, *Changing Britain, changing lives. Three generations at the turn of the century.* Institute of Education, University of London, London, 2003.

Department of Education and Science. Children and their primary schools: A report of the Central Advisory Council for Education (England). Technical report, 1967.

Department of Education and Science. Burnham Further Education Committee Grading Courses. Technical report, HMSO, London, 1972.

Richard Dickens and Alan Manning. Has the national minimum wage reduced UK wage inequality? *Journal of the Royal Statistical Society Series A*, 167(4):613–626, 2004. Statistics in Society.

John DiNardo, Nicole M. Fortin, and Thomas Lemieux. Labour market institutions and the distribution of wages, 1973-1992:A semiparametric approach. *Econometrica*, 64(5):1001–1044, September 1996.

Richard Disney and Steven Webb. Why are there so many long term sick in Britain? *The Economic Journal*, 3(1):252–262, March 1991.

P. J. Dolton and G. H. Makepeace. Marital status, child rearing and earnings differentials in the graduate labour market. *The Economic Journal*, 97(388):897–922, December 1987a.

P. J. Dolton and G. H. Makepeace. Interpreting sample selection effects. *Economic Letters*, 24:373–379, April 1987b.

Peter Dolton, Heather Joshi, and Gerry Makepeace. Unpacking unequal pay between men and women across cohort and lifecycle. CLS Cohort Studies Working Paper 2, Centre for Longitudinal Studies, Institute of Education, London, October 2002.

Peter Dolton, Oscar Marcenaro-Gutierrez, and Ali Skalli. Gender wage differentials in Europe. In Peter Dolton, Rita Asplund, and Erling Barth, editors, *Education and inequality across Europe*, chapter 9. Edward Elgar Pub, 2009.

Gabriele B. Durrant. Missing data methods in official statistics in the United Kingdom: some recent developments. *Allgemeines Statistiches Archiv*, 90:577–593, November 2006.

Jane Elliott and Peter Shepherd. Cohort profile: 1970 British Birth Cohort (BCS70). *International Journal of Epidemiology*, 35(4):836–843, August 2006.

John Ermisch. The relevance of the 'Easterlin Hypothesis' and the 'New Home Economics' to fertility movements in Great Britain. *Population Studies*, 33(1):39–78, March 1979.

John F. Ermisch and Robert E. Wright. Wage offers and full-time and part-time employment by British women. *The Journal of Human Resources*, 28(1):111–133, Winter 1993.

Giulia Faggio and Stephen Nickell. The rise in inactivity among adult men. In Richard Dickens, Paul Gregg, and Jonathan Wadsworth, editors, *The labour market under New Labour: The state of working Britain*, chapter 3, pages 40–52. Palgrave Macmillan, 2003.

Elsa Ferri. Forty years on: Professor Neville Butler and the British Birth Cohort Studies. *Paediatric and Perinatal Epidemiology*, 12(Supplement 1):31–44, 1998.

Elsa Ferri, John Bynner, and Michael Wadsworth, editors. *Changing Britain, changing lives. Three generations at the turn of the century.* Institute of Education, University of London, London, 2003.

Eirini Flouri and Denise Hawkes. Ambitious mothers - successful daughters: Mothers' early expectations for children's education and children's earnings and sense of control in adult life. *British Journal of Educational Psychology*, 78:411–433, 2008.

Nancy Folbre. *Who pays for the kids? gender and the structures of constraint.* Routledge, London, 1994.

Stephen Fothergill. The true scale of the regional problem in the UK. *Regional Studies*, 35(3):241–246, 2001.

Joanna Gomulka and Nicholas Stern. The employment of married mothers in the United Kingdom, 1970-83. *Economica*, 57(226):171–199, May 1990.

Maarten Goos and Alan Manning. Lousy and lovely jobs: The rising polarization of work in Britain. *The Review of Economics and Statistics*, 89(1):118–133, February 2007.

Christine A. Greenhalgh and Mark B. Stewart. The occupational status and mobility of British men and women. *Oxford Economic Papers*, 37(1):40–71, March 1985.

Paul Gregg. The impact of youth unemployment on adult unemployment in the NCDS. *The Economic Journal*, 111(475):F626–F653, November 2001. Features.

Paul Gregg, Maria Gutiérrez-Domènech, and Jane Waldfogel. The employment of married mothers in Great Britain, 1974-2000. *Economica*, 75(296):842–864, November 2007.

Mary Gregory and Sara Connolly. Feature: The price of reconciliation: Part-time work, families and women's satisfaction. *The Economic Journal*, 118(526):F1–F7, February 2008. Feature: women's part-time work.

Mary Gregory and Robert Jukes. Unemployment and subsequent earnings: Estimating scarring among British men 1984-94. *The Economic Journal*, 111(475):F607–F625, November 2001. Features.

Damian Grimshaw and Jill Rubery. Gender and internal labour markets. In Jane Humphries and Jill Rubery, editors, *The economics of equal opportunity.* Equal Opportunities Commission, Manchester, 1995.

Reuben Gronau. Wage comparisons - a selectivity bias. *The Journal of Political Economy*, 82(6):1119–1143, December 1974.

Reuben Gronau. Sex-related wage differentials and women's interrupted labor careers - the chicken or the egg. *Journal of Labor Economics*, 6(3):277–301, 1988.

Maria Gutiérrez-Domènech and Brian Bell. Female labour force participation in the UK: evolving characteristics or changing behaviour? Working Paper no. 221, Bank of England, 2004.

Susan Harkness. The gender earnings gap: Evidence from the UK. *Fiscal Studies*, 17 (2):1–36, 1996.

Denise Hawkes and Ian Plewis. Modelling non-response in the National Child Development Study. *Journal of the Royal Statistical Society Series A*, 169(3):479–491, 2006. Statistics in Society.

James J. Heckman. The common structure of statistical models of truncation, sample selection and limited dependent variables and a simple estimator for such models. *The Annals of Economic and Social Measurement*, 5:475–492, 1976.

James J. Heckman. Sample selection bias as a specification error with an application to the estimation of labor supply functions. NBER Working Paper 172, National Bureau of Economic Research, March 1977.

James J. Heckman. Sample selection bias as a specification error. *Econometrica*, 47(1): 153–162, January 1979.

James J. Heckman and Salvador Navarro-Lozano. Using matching, instrumental variables and control functions to estimate economic choice models. *The Review of Economics and Statistics*, 86(1):30–57, February 2004.

John Hills. *Inequality and the state*. Oxford University Press, Oxford, 2004.

Sue Himmelweit. Caring labor. *Annals of the American Academy of Political and Social Science*, 561:27–38, 1999.

Barry T. Hirsch and Edward J. Schumacher. Match bias in wage gap estimates due to earnings imputation. *Journal of Labor Economics*, 22(3):689–722, 2004.

Jane Humphries and Jill Rubery. *The economics of equal opportunity*. Equal Opportunities Commission, Manchester, 1995.

Ben Jann. A Stata implementation of the Blinder-Oaxaca decomposition. Eth zurich sociology working paper no. 5, Institute of Technology Zurich, May 2008.

Andrew Jenkins, Heather Joshi, and Mark Killingsworth. Educational attainment, labour market conditions and unobserved heterogeneity: The timing of first and higher-order births in Britain. Working Paper 2008/12, Centre for Longitudinal Studies, Institute of Education, November 2008.

Joint Committee of the Royal College of Obstetricians and Gynaecologists and the Population Investigation Committee. *Maternity in Great Britain.* Oxford University Press, Oxford, 1948.

Heather Joshi. Production, reproduction and education: women, children and work in a British perspective. *Population and Development Review*, 28(3):445–474, September 2002.

Heather Joshi and Pierella Paci. *Unequal pay for women and men: Evidence from the British Birth Cohort Studies.* The MIT Press, Cambridge, Massachusetts, 1998. with Gerald Makepeace and Jane Waldfogel.

Heather Joshi, Gerry Makepeace, and Peter Dolton. More or less unequal? Evidence on the pay of men and women from the British Birth Cohort Studies. *Gender, Work and Organization*, 14(1):37–55, January 2007.

Heather E. Joshi and Marie-Louise Newell. Pay differences between women and men: longitudinal evidence from the 1946 birth cohort. DP156, Centre for Economic Policy Research, March 1987.

Heather E. Joshi and Marie-Louise Newell. Pay differentials and parenthood: Analysis of men and women born in 1946. Technical report, Institute of Employment Research, 1989.

Heather E. Joshi, Richard Layard, and Susan J. Owen. Why are more women working in Britain? *Journal of Labor Economics*, 3(1):S147–S176, January 1985.

Chinhui Juhn. Decline of male labor participation: the role of declining market opportunities. *The Quarterly Journal of Economics*, 107(1):79–121, February 1992.

Chinhui Juhn, Kevin M. Murphy, and Brooks Pierce. Wage inequality and rise in returns to skill. *The Journal of Political Economy*, 101(3):410–442, June 1993.

Lawrence F. Katz and David H. Autor. Changes in the wage structure and earnings inequality. In Orley Ashenfelter and David Card, editors, *Handbook of labor economics*, volume 3A, pages 1463–1555. Elsevier Science B.V., Amsterdam, 1999.

Mark Killingsworth. Heterogeneous preferences, compensating wage differentials and comparable worth. *The Quarterly Journal of Economics*, 102(4):727–742, November 1987.

Dylan Kneale and Heather Joshi. Postponement and childlessness: Evidence from two British cohorts. *Demographic Research*, 19(58):1936–1968, November 2008.

Diana Kuh and Michael Wadsworth. Childhood influences on adult male earnings in a longitudinal study. *The British Journal of Sociology*, 42(4):537–555, December 1991.

Diana Kuh, Jeremy Head, Rebecca Hardy, and Michael Wadsworth. The influence of education and family background on women's earnings in midlife: evidence from a British national birth cohort study. *British Journal of Sociology of Education*, 18(3): 385–405, 1997.

Edwin Leuven and Barbara Sianesi. PSMATCH2: Stata module to perform full Mahalanobis and propensity score matching, common support graphing, and covariate imbalance testing, version 3.1.5. Technical report, 2003. http://ideas.repec.org/c/boc/bocode/s432001.html.

H. Gregg Lewis. Comments on selectivity biases in wage comparisons. *The Journal of Political Economy*, 82(6):1145–1155, December 1974.

Assar Lindbeck and Dennis J. Snower. Insiders versus outsiders. *The Journal of Economic Perspectives*, 15(1):165–188, Winter 2001.

Roderick J.A. Little and B. Rubin, Donald. *Statistical analysis with missing data*. John Wiley & Sons, Inc., New York, 2nd edition, 2002.

Mats Lundahl and Eskil Wadensjö. Economic theories of discrimination. In *Unequal treatment: a study of the neo-classical theory of discrimination*, chapter 2, pages 8–72. New York University Press, Croom Helm, New York, London and Sydney, 1984.

Stephen Machin. Wage inequality since 1975. In Richard Dickens, Paul Gregg, and Jonathan Wadsworth, editors, *The labour market under New Labour: The state of working Britain 2003*, chapter 12, pages 191–200. Palgrave Macmillan, Basingstoke, Hampshire and New York, 2003.

Stephen Machin, Alan Manning, and Lupin Rahman. Where the minimum wage bites hard: the introduction of the UK National Minimum Wage to a low wage sector. Discussion Paper No.544, Centre for Economic Performance, 2002.

Susan Macran, Shirley Dex, and Heather Joshi. Employment after childbearing: A survival analysis. *Work, Employment and Society*, 10(2):273–296, June 1996.

Gerald Makepeace, Pierella Paci, Heather Joshi, and Peter Dolton. How unequally has equal pay progressed since the 1970s? A study of two British cohorts. *The Journal of Human Resources*, 34(3):534–556, Summer 1999.

Gerry Makepeace, Peter Dolton, Laura Woods, Heather Joshi, and Fernando Galinda-Rueda. From school to the labour market. In Elsa Ferri, John Bynner, and Michael Wadsworth, editors, *Changing Britain, changing lives. Three generations at the turn of the century*, chapter 2, pages 29–70. Institute of Education, University of London, London, 2003.

Alan Manning. The Equal Pay Act as an experiment to test theories of the labour market. *Economica*, 63(250):191–212, May 1996.

Alan Manning. Movin' on up: Interpreting the earnings-experience profile. *Bulletin of Economic Research*, 52(4):261–295, 2000.

Alan Manning. *Monopsony in motion: imperfect competition in labor markets.* Princeton University Press, Princeton NJ, 2003.

Alan Manning and Barbara Petrongolo. The part-time penalty for women in Britain. *The Economic Journal*, 118(526):F28–F51, February 2008. Feature: women's part-time work.

Alan Manning and Joanna Swaffield. The gender gap in early-career wage growth. *The Economic Journal*, 118(530):983–1024, July 2008.

Charles F. Manski. Anatomy of the selection problem. *The Journal of Human Resources*, 24(3):343–360, Summer 1989.

John Micklewright and Sylke V. Schnepf. How reliable are income data collected with a single question? IZA Discussion Paper No. 3177, Institute for the Study of Labor (IZA), November 2007.

Jacob Mincer. Investment in human capital and personal income distribution. *The Journal of Political Economy*, 66(4):281–302, August 1958.

Jacob Mincer and Haim Ofek. Interrupted work careers: depreciation and restoration of human capital. *The Journal of Human Resources*, 17(1):3–24, Winter 1982.

Jacob Mincer and Solomon Polachek. Family investments in human capital: earnings of women. *The Journal of Political Economy*, 82(2):S76–S108, April 1974. Part 2.

Casey B. Mulligan and Yona Rubinstein. Selection, investment and women's relative wages over time. *The Quarterly Journal of Economics*, 123(3):1061–1110, August 2008.

Michael Murphy. The contraceptive pill and women's employment as factors in fertility change in Britain 1963-1980: a challenge to the conventional view. *Population Studies*, 47(2):221–243, July 1993.

Michal Myck and Gillian Paull. The role of employment experience in explaining the gender wage gap. WP04/16, The Institute for Fiscal Studies, July 2004.

Gunnar Myrdal. *An American dilemma*. Oxford University Press, Oxford, 1944.

Gad Nathan. A review of sample attrition and representativeness in three longitudinal surveys. Methodology Series No. 13, Government Statistical Service, 1999.

Henry Neuburger. Unemployment: are wages to blame? a critique of the 'priced-out' hypothesis. Discussion series, Low Pay Unit, 1984.

Shoshana Neuman and Ronald L. Oaxaca. Wage decompositions with selectivity corrected wage equations: a methodological note. *Journal of Economic Inequality*, 2: 3–10, 2004.

David Neumark. Employers' discriminatory behavior and the estimation of wage discrimination. *The Journal of Human Resources*, 23(3):279–295, Summer 1988.

Martha Nussbaum. Adaptive preferences and women's options. *Economics and Philosophy*, 17:67–88, 2001. Symposium on Amartya Sen's philosophy.

Ronald Oaxaca. Male-female wage differentials in urban labor markets. *International Economic Review*, 14(3):693–709, March 1973.

Ronald Oaxaca and R. Ransom, Michael. On discrimination and the decomposition of wage differentials. *Journal of Econometrics*, 61:5–21, March 1994.

Office for National Statistics. *Birth statistics, Review of the National Statistician on births and patterns of family building in England and Wales, 2007*. Series FM1 No.36. National Statistics, 2008a.

Office for National Statistics. *Social Trends 2008*. Number No.38 in Social Trends. National Statistics, 2008b. Editors: Abigail Self and Linda Zealey.

Office for National Statistics. *Social Trends 2009*. Number No.39 in Social Trends. National Statistics, 2008c. Editors: Matthew Hughes, Jenny Church and Linda Zealey.

Walter Y. Oi. Labour as a quasi-fixed factor. *Journal of Political Economy*, 70:538–555, 1962.

Claudia Olivetti and Barbara Petrongolo. Unequal pay or unequal employment? a cross-country analysis of gender gaps. *Journal of Labor Economics*, 26(4):621–654, October 2008.

Edmund S. Phelps. The statistical theory of racism and sexism. *The American Economic Review*, 62(4):659–661, September 1972.

Michael J. Piore and Peter B. Doeringer. *Internal labor markets and manpower analysis*. Lexington Books, Lexington: MA, 1971.

Ian Plewis and Constantinos Kallis. Changing economic circumstances in childhood and their effects on subsequent educational and other outcomes. Working Paper Number 49, Department for Work and Pensions, February 2008.

Ian Plewis, Lisa Calderwood, Denise Hawkes, and Gad Nathan. National Child Development Survey and 1970 British Cohort Study Tehnical Report: Changes in the NCDS and BCS70 populations and samples over time. Technical report, Centre for Longitudinal Studies, Institute of Education, October 2004.

Solomon William Polachek. Occupational self-selection: a human capital approach to differences in occupational structure. *The Review of Economics and Statistics*, 63(1): 60–69, February 1981.

Chris Power and Jane Elliott. Cohort profile: 1958 British birth cohort (National Child Development Study). *International Journal of Epidemiology*, 35(1):34–41, February 2006.

Patrick A. Puhani. The Heckman correction for sample selection and its critique. *Journal of Economic Surveys*, 14(1):53–68, 2000.

Helen Robinson. Are you experienced? British evidence on age-earnings profiles. *Applied Economics*, 35(9):1101–1115, 2003a.

Helen Robinson. Gender and labour market performance in the recovery. In Richard Dickens, Paul Gregg, and Jonathan Wadsworth, editors, *The labour market under*

New Labour: The state of working Britain, chapter 15, pages 232–248. Palgrave Macmillan, 2003b.

Joan Robinson. *The economics of imperfect competition.* Macmillan, second edition edition, 1969. First edition, 1933.

Paul R. Rosenbaum and Donald B. Rubin. The central role of the propensity score in observational studies for causal effects). *Biometrika*, 70(1):41–55, April 1983.

Royal Commission on Equal Pay. *Report of the Royal Commission on Equal Pay 1944-46.* HMSO, London, October 1946. Cmd. 6937.

B. Rubin, Donald. *Multiple imputation for nonresponse in surveys.* John Wiley & Sons, Inc., Hoboken, New Jersey, 1987. Wiley series in probability and mathematic statistics.

Donald B. Rubin. Inference and missing data. *Biometrika*, 63(3):581–592, December 1976.

Christopher J. Ruhm. The economic consequences of parental leave mandates: lessons from Europe. *The Quarterly Journal of Economics*, 113(1):285–317, February 1998.

Innis G. Sande. Imputation in surveys: coping with reality. *The American Statistician*, 36(3):145–152, August 1982.

Malcolm Sawyer. The operation of labour markets and the economics of equal opportunities. In Jane Humphries and Jill Rubery, editors, *The economics of equal opportunity*, chapter 2. Equal Opportunities Commission, Manchester, UK, 1995.

Ingrid Schoon, John Bynner, Heather Joshi, Samantha Parsons, Richard D. Wiggins, and Amanda Sacker. The influence of context, timing, and duration of risk experiences for the passage from childhood to midadulthood. *Child Development*, 73(5):1486–1504, October 2002.

Chris Skinner, Nigel Stuttard, Gabriele B. Durrant, and J. Jenkins. The measurement of low pay in the UK Labour Force Survey. *Oxford Bulletin of Economics and Statistics*, 64:653–676, December 2002. Special Issue: Issue supplement.

Deborah Smeaton. Work return rates after childbirth in the UK - trends, determinants and implications: a comparison of cohorts born in 1958 and 1970. *Work, Employment and Society*, 20(1):5–25, 2006.

James P. Smith and Finis R. Welch. Closing the gap: forty years of economic progress for blacks. Department of Labor, Washington, D.C., Rand Corporation, Santa Monica, CA, February 1986.

Kate Smith. National Child Development Study 5: derived variables 1. Technical report, Centre for Longitudinal Studies, Institute of Education, London, UK, 1991.

Thomas Sowell. *Markets and minorities*. Basil Blackwell, Oxford, 1981.

Michael A. Spence. *Market signaling*. Harvard University Press, Cambridge, Mass., 1974.

Mark B. Stewart. On least squares estimation when the dependent variable is grouped. *The Review of Economic Studies*, 50(4):737–753, 1983.

Mark B. Stewart and Joanna K. Swaffield. The employment effects of the national minimum wage. *Economic Journal*, 114:C110–C116, 2004.

Mark B. Stewart and Joanna K. Swaffield. The other margin: Do minimum wages cause working hours adjustments for low-wage workers? *Economica*, 75:148–167, 2008.

Joseph G Stigler. The economics of information. *The Journal of Political Economy*, 69: 213–225, 1961.

Wing Suen. Decomposing wage residuals: Unmeasured skill or statistical artifact? *Journal of Labor Economics*, 15(3):555–566, July 1997.

Joanna K. Swaffield. Estimates of the impact of labour market attachment and attitudes on the female wage. *The Manchester School*, 75(3):349–371, June 2007.

Francis Vella. Models with sample selection bias: a survey. *The Journal of Human Resources*, 33(1):127–169, Winter 1998.

Michael Wadsworth, Diana Kuh, Marcus Richards, and Rebecca Hardy. Cohort profile: The 1946 national birth cohort (MRC National Survey of Health and Development). *International Journal of Epidemiology*, 35(1):49–54, February 2006.

Michael E. J. Wadsworth. *The imprint of time: childhood history and adult life*. Oxford University Press, Oxford, UK, 1991.

Michael E. J. Wadsworth, S.L. Butterworth, D.J Kuh, M. Richards, C. Langenberg, W.S. Hilder, and M. Connor. The life course prospective design: an example of benefits and problems associated with study longevity. *Social Science & Medicine*, 57:2193–2205, 2003.

Jane Waldfogel. Understanding the "family gap" in pay for women with children. *Journal of Economic Perspectives*, 12(1):137–156, Winter 1998.

Sidney Webb. The alleged differences in the wages paid to men and to women for similar work. *The Economic Journal*, 1(4):635–662, December 1891.

Finis Welch. The employment of black men. *Journal of Labor Economics*, 8(1):S26–S74, January 1990. Part 2: Essays in honor of Albert Rees.

Robert E. Wright and John F. Ermisch. Gender discrimination in the British labour market: A reassessment. *The Economic Journal*, 101(406):508–522, May 1991.

A. Zabalza and J. L. Arrufat. The extent of sex discrimination in Great Britain. In A. Zabalza and Z. Tzannatos, editors, *Women and equal pay: The effects of legislation on female employment and wages in Britain*, chapter 5, pages 70–94. Cambridge University Press, 1985.

A. Zabalza and Z. Tzannatos. *Women and equal pay: The effects of legislation on female employment and wages in Britain.* Cambridge University Press, 1985.